UNIVERSAL
LIBRARY

OU_216910

UNIVERSAL
LIBRARY

The Drinched Book

text fiy book

PAGE MISSING

OSMANIA UNIVERSITY LIBRARY

Call No. Accession No.

Author

Title

This book should be returned on or before the date last marked below

ISAAC NEWTON

1642-1727

ISAAC NEWTON

1642-1727

A MEMORIAL VOLUME
EDITED FOR THE
MATHEMATICAL ASSOCIATION
BY
W. J. GREENSTREET

LONDON
G. BELL AND SONS, LTD
1927

PRINTED IN GREAT BRITAIN BY ROBERT MACLEHOSE AND CO. LTD.
THE UNIVERSITY PRESS, GLASGOW.

PREFACE

A WORD of explanation as to the genesis of this volume seems to be necessary.

In January, 1926, the Council of the Mathematical Association suggested that the bicentenary of the death of Newton should be commemorated by a special number of the *Mathematical Gazette*.

The response to the Editor's appeal for contributions was generous and varied. It soon became clear both that the limits of a single number would be far exceeded, and that interest in the material would not be confined to members of the Association. The Council therefore decided to render the book independent, and not to include it among the volumes of the *Gazette*, even as a volume *extra numerum*.

The Council, on behalf of the members of the Mathematical Association, tender their heartiest thanks to those who have done so much to make this memorial a notable addition to Newtonian literature.

But while the Council tenders its thanks to all the contributors, there are some of whom special mention must be made for contributing contemporary material.

The late Dr. J. L. E. Dreyer had promised papers (*a*) on Newton and the Comet of 1680, for which he wrote, " I have the material ready, including some I found in the library of Corpus Christi here some years ago," and (*b*) on astronomical instruments. The following extract from his daughter's letter to the Editor tells its own story, and it is indeed difficult to find words appropriate to an occasion of such pathos and revealing so high an appreciation of the sanctity of a promise. Mrs. Shaw-Hamilton wrote (Sept. 1):

" . . . His illness has taken a very serious turn, and he has now only a *very* short time to live. He asks me to write and tell you this, and how very much he regrets that after all he will not be able to write on the subjects he had arranged with you to undertake. He asks me to send you . . . copies of letters from Newton to Flamsteed, from Corpus Christi College, Oxford, and to say how much he hopes that good use will be made of them." A few days later he had passed away.

The Council must also acknowledge the courtesy of the Trustees of the Pierpont Morgan Library, New York, for allowing Prof. David

Eugene Smith to make use, for the purposes of this memorial, of a note-book kept by Newton as a young man.

Prof. D. E. Smith undertook the toil of deciphering the contents, and provided us with reproductions from its pages. It may be thought that the contents of the note-book are hardly mathematical enough to find a place in the *Gazette.* But this is a Newton number. The existence of the note-book is almost unknown. And now it is possible, and for this we express our warmest thanks to Prof. D. E. Smith, for the first time to publish material telling something of Newton and his interests when on the threshold of his manhood.

Nor must we omit to refer to the fine feeling shown by Prof. D. E. Smith in offering to the *Gazette* the fruit of his labours :

" It is entirely proper that all Newtoniana should be published in England first, in case this can be done."

To Mr. D. C. Fraser their thanks are tendered for his researches in the Newton MSS. in the possession of Trinity College, Cambridge, the outcome of which will be read in his able paper on " Interpolation."

The thanks of the Council are also due to Prof. H. F. Baker, F.R.S., the fortunate possessor of an engraving of a Newton portrait, elsewhere described as the most noteworthy of all the portraits for "real humanity." The reproduction of this engraving forms our frontispiece.

The reproduction of the Abbey Tomb was secured by the skill of our printers from photography especially procured by our secretary, Mr. C. Pendlebury.

For the remaining reproductions our readers are indebted to the courtesy of the Grantham Public Library, to the kindness of Messrs. A. J. Tate and J. A. Holden, the Head Master and the Senior Mathematical Master respectively of Newton's old school, the King's School, Grantham, and to Mr. Wilfrid Bond, F.R.I.B.A., of Grantham.

The Council must acknowledge its indebtedness to Messrs. MacLehose & Co. for the courteous readiness with which they have placed their wide experience at the disposal of the Association in the preparation of this volume.

Finally, the Editor must express to Prof. E. H. Neville his gratitude for invaluable assistance rendered, especially during an illness which would otherwise have delayed the appearance of the book.

CONTENTS

PAGE

Absolute Rotation. Prof. A. S. Eddington, M.A., M.Sc., F.R.S. - 1

The Forms of Carbon and Chemical Affinity. Prof. H. E. Armstrong, Ph.D., LL.D., D.Sc., F.R.S. - - - - - - - - 5

Two Unpublished Documents of Sir Isaac Newton. Prof. D. E. Smith, Ph.D., LL.D. - - - - - - - - - - - - 16

Letters from Newton in Corpus Christi College, Oxford. Communicated by the late J. L. E. Dreyer, Ph.D., D.Sc. - - 35

Newton and Interpolation. D. C. Fraser, M.A. - - - - - 45

Newton's Work in Optics. Prof. E. T. Whittaker, Sc.D., F.R.S. - 70

Newton's Problem of the Solid of Least Resistance. Prof. A. R. Forsyth, Sc.D., LL.D., F.R.S. - - - - - - - 75

Newton's Work on the Theory of the Tides. Prof. J. Proudman, D.Sc., F.R.S. - - - - - - - - - - - - 87

Newton's Contribution to the Geometry of Conics. The Rev. J. J. Milne, M.A. - - - - - - - - - - - - 96

Newton on Plane Cubic Curves. Prof. H. Hilton, D.Sc. - - 115

Newton and the Art of Discovery. J. M. Child, B.A. - - - - 117

Newton's Influence on Method in the Physical Sciences. Prof. A. E Heath, Ph.D. - - - - - - - - - - 130

Plagiarism in the Seventeenth Century, and Leibniz. Prof. L. J. Russell, M.A. - - - - - - - - - - - 134

The Contemporary Significance of Newton's Metaphysics. Prof. E. A. Burtt, Ph.D. - - - - - - - - - - - 137

Newton and his Homeland; the Haunts of his Youth. J. A. Holden, M.A. - - - - - - - - - - - - - 141

Trinity College in the Time of Newton. Prof. G. N. Watson, Sc.D., F.R.S. - - - - - - - - - - - - 144

A Newton Bibliography. H. Zeitlinger - - - - - - 148

Portraits of Sir Isaac Newton. Prof. D. E. Smith, Ph.D., LL.D. - 171

The Portrait Medals of Newton. Prof. D. E. Smith, Ph.D., LL.D. 179

Epitaph - - - - - - - - - - - - - - - 181

LIST OF PLATES

An Engraving after Gandy - - - - - - - - *frontispiece*

Newton's Tomb in Westminster Abbey - - - - - - - - viii

The Manor House, Woolsthorpe-by-Colsterworth - - - - - 45

Theed's Statue at Grantham - - - - - - - - - - - 75

An Engraving after Vanderbank in the Grantham Museum - - 117

The Interior of the Old School, Grantham - - - - - - - 141

Carved Inscriptions in The King's School, Grantham - - - - 142

Roubiliac's Statue in Trinity - - - - - - - - - - 144

A Portrait, artist unknown - - - - - - - - - - 170

[William Kent] was employed in erecting two . . . monuments in the Abbey, which outshine every other name, however illustrious by rank or heroic action. One was but a cenotaph . . . —the statue of Shakespeare in Poets' Corner. But the other was to celebrate the actual interment of the only dust of unquestionably world-wide fame that the floor of Westminster covers—of one so far raised above all the political or literary magnates by whom he is surrounded, as to mark an era in the growth of the monumental history of the whole building. On March 28, 1727, the body of Sir Isaac Newton, after lying in state in the Jerusalem Chamber, where it had been brought from his death-bed in Kensington, was attended by the leading members of the Royal Society, and buried at the public cost in the spot in front of the Choir, which, being " one of the most conspicuous in the Abbey, had been previously refused to various noblemen who had applied for it." Voltaire was present at the funeral.

. . . On the gravestone are written the words, which here acquire a significance of more than usual solemnity—

*Hic depositum quod mortale fuit Isaaci Newtoni.**

On the monument was intended to have been inscribed the double epitaph of Pope:

ISAACUS NEWTONIUS
Quem Immortalem
Testantur *Tempus, Natura, Coelum*:
Mortalem
Hoc marmor fatetur.

Nature and Nature's laws lay hid in night:
God said, *Let Newton be!*—and all was light.

The actual inscription agrees with the actual monument—the one in words, the other in marble allegory, a description of Newton's discoveries, closing with the summary:

Naturae, antiquitatis, Sanctae Scripturae sedulus, sagax, fidus interpres, Dei O.M. majestatem philosophiâ asseruit; Evangelii simplicitatem moribus expressit. Tibi gratulentur mortales, tale tantumque exstitisse humani generis decus.—*Historical Memorials of Westminster Abbey*, by Dean Stanley (1886), pp. 293-4.

---

* Johnson had intended *Isaacus Newtonius, legibus naturae investigatis, hic quiescit.*

ABSOLUTE ROTATION.

By Prof. A. S. Eddington, M.A., M.Sc., F.R.S.

In the Newtonian scheme of dynamics translation was relative but rotation was absolute. The introduction of a universal aether temporarily restored absolute translation, motion through the aether being an "absolute motion" from the point of view of a physical scheme. It is well known that Einstein's special theory restored complete relativity of uniform translation and that his general theory extended relativity to non-uniform motion. But I think most students of relativity feel uncertain as to what has happened to absolute rotation on the new theory. It may be appropriate in connection with a Newton celebration to try to explain what is the modern position of this problem which is generally regarded as having originated with Newton. I should add that I am not sure how far the following views will be acceptable to other writers on the subject.

We are not concerned with absolute motion in a philosophical sense. I do not know of any authoritative definition of what is to be regarded as absolute motion in the physical sense; but the points at issue seem to arise in the following way. *Position* signifies position relative to recognisable landmarks, or relative to a frame of reference defined with respect to recognisable landmarks. Ultimate reference to landmarks is essential to our notion of position, and if any one speaks of position in a medium not differentiated in a recognisable way he is merely stringing words together which convey no meaning. *Velocity, acceleration, rotation* are derived from position, and therefore as ordinarily defined presuppose recognisable landmarks to which they are relative. The question is whether any quantities occur in nature, involving no reference to recognisable landmarks, which *by a reasonable extension of the definitions* might be described as velocity, acceleration, rotation.

In Einstein's theory there is no candidate for the title of absolute velocity; so that part of the question drops. But there are plausible claimants for the title of absolute acceleration and rotation. It is clear that no hard and fast decision can be given on a question of taste, viz. what constitutes a reasonable extension of a definition.

The complication arises because a *relative acceleration* can be produced by an *absolute force.** There is a relative acceleration of a train and a station platform which (because it is relative) should not be attributed to one rather than the other. But the force causing it is applied to the train. Any observer, no matter what his frame of reference, can detect the molecular impacts which produce the change of velocity. There is a very natural tendency to locate the acceleration in the body which experiences the cause of the acceleration, and say that the train experiences an absolute acceleration and the station no absolute acceleration. The state of stress which exists in the train but not in the platform is an absolute *disturbance*; the observed acceleration is one of the visible consequences of the disturbance. It is suggested that the acceleration must definitely belong to the disturbed train, and not to some neighbouring (or distant) body which is not causally affected by the disturbance.

---

* Relative acceleration can also be produced by a purely relative force, viz. gravitation.

By dragging in the disturbance we should violate the definition of acceleration as a purely kinematical quantity; but some modification of the definition must be contemplated, otherwise the question of absolute acceleration would not arise. The acceleration of a body with respect to the natural track or geodesic in space-time is directly correlated to an absolute force, and has considerable claims to the title absolute acceleration, but there are countervailing objections. Firstly, relative acceleration is not always the difference of two absolute accelerations; for example, there is a relative acceleration between the earth and the sun, although both bodies are pursuing geodesics and would have zero absolute acceleration according to the proposed definition. The association of disturbance and acceleration is occasional and not universal. Secondly, it upsets common practice; we should have to say that a falling stone has no acceleration, but *we* have an absolute acceleration upwards, since it is we who experience the absolute disturbance of molecular bombardment which causes the relative acceleration g.

The same considerations apply to rotation; but the claim of absolute rotation is greatly strengthened by the fact that the relative rotation is always equal to the difference of the two absolute rotations to within the utmost accuracy attainable by observation. There is still a theoretical discrepancy, and logically it should make no difference whether the discrepancy is large as in astronomical accelerations, or below the limit of observation as in astronomical rotations; but a practical point of this kind naturally influences us in a decision on a matter not of logic but of taste. By dynamical experiments (*e.g.* Foucault's pendulum) we can find a quantity which claims the title of the earth's absolute rotation. Determining in this way a frame claiming the title of zero rotation, we arrive at the Galilean or inertial frame. Note that the frame is derived by dynamical experiments and not by reference to landmarks. Here the complication arises that the stars, if used as landmarks, define a frame which agrees with the earth's inertial frame within the errors of observation.* I take this to mean that the inertial frame in the neighbourhood of the earth extends universally with insignificant change, so that the motions of the stars conform to it and exhibit it.†

The followers of Mach have felt that this leads to a difficulty in regard to the principles of causation. I take it that their objection is this. Suppose that there is just one planet alone in the universe, so that there is no landmark to define a constant direction in space. Orientation is then meaningless. Change of orientation is therefore meaningless. How then can a meaningless change of orientation (a rotation) cause the planet to take a spheroidal rather than a spherical shape? (The shape can, of course, be tested absolutely by triangulation of the planet.)

It was to meet this difficulty that Einstein proposed his spherical world some time after the main part of his general theory of relativity. According to his theory, if the planet were really the only matter in the universe the change *would not occur*. The distortion into a spheroid is attributed to dynamical forces exerted on the planet by large quantities of matter distributed through the spherical world; and the amount of distortion then depends only on the relative rotation of the planet and the world-matter. Thus the so-called absolute rotation of the earth is reduced to a relative rotation attributable either to the earth or the world-matter indifferently. I do not like this hypothesis. Although I believe that the introduction of spherical (or elliptical) space was an important advance, I prefer the modified theory of it afterwards proposed by de Sitter, and I would not connect spherical space with the problem of absolute rotation.

---

* Apart from a possible "rotation of the stellar system" there is a theoretical difference of 1″·94 per century due to the Schouten-Fokker precession of the earth's inertial frame.

† The inertial frame may be defined as the whole group of Galilean frames connected by the Lorentz transformation. The velocity of a star relative to any of these frames must be less than 300,000 km. per sec. The velocity in arc of a distant star, even if the linear velocity were 300,000 km. per sec., would be very small compared with the diurnal motion of the heavens, so that within this limit of accuracy the star "exhibits" the inertial frame in its neighbourhood for the purposes of a determination of the earth's rotation.

It seems to me that the Machian difficulty is a pure misunderstanding. Taking the ordinary gravitational equations of Einstein (outside and inside matter), there exist an infinite number of steady solutions representing different distributions of mass, momentum and stress. Among them is a spherically symmetrical distribution of mass and stress. This represents a planet ordinarily described as non-rotating. Other solutions are spheroidal distributions of mass with tangential stresses. These represent rotating planets. There is no question of causation of the tangential stresses ; they are as much a part of the specification of the particular solution that is under consideration as the mass of the planet is. The rotating and non-rotating planet are different types of singularity in the gravitational field, which primarily have nothing to do with orientation, since they are definable whether there is anything to define orientation or not. The various types of singularities can be specified by an invariant M and a vector Ω. By following out the theory and studying the behaviour of a test body in the space around the singularity, we can find experimental interpretations for M and Ω. It turns out that M is the mass.

Suppose that the test body is another planet with a vector Ω'. Then it can be deduced from the equations that the relative angular velocity of the two planets is approximately $\omega = \Omega - \Omega'$. Hence, if the test body is kept constant, $\delta\omega = \delta\Omega$. This is the origin of the interpretation of the absolute quantity Ω as a rotation. Again, if we have a number of *very distant* test bodies (whose velocities will necessarily be less than the velocity of light), it is found that the angular velocity of the planet relative to directions fixed with respect to the distribution of these bodies is approximately equal to Ω. But although Ω can usually be interpreted as a rotation in this way, it does not cease to be significant when that interpretation is not available. The spheroidal shape of the body depends on Ω, and, in so far as it can be said to be caused, is caused by Ω ; but we see that that does not mean that it is caused by a change of orientation.

I think the misunderstanding has arisen, because in the idea of Mach mass was something more fundamental than momentum or stress. That is directly contrary to Einstein's theory, according to which mass, momentum and stress are coequal components of the same tensor. Thus the Machian view would not allow any causative influence to momentum as such ; any effects must be attributed to the change in the distribution of mass which usually (but not invariably) accompanies the occurrence of momentum. Since momentum is related to change of mass-distribution, and stress to change of momentum-distribution, by laws of conservation, the fallacy does not usually betray itself in practice. But to argue that if the change of mass-distribution happens to be undetectable, then momentum is unable to produce its other natural effects, is to put the cart before the horse. A simple illustration is afforded by a disc made of continuous matter spinning in its own plane ; here there is no change of position of matter,* but the disc is distinguished from a non-rotating disc by its possession of angular momentum, and the momentum can produce its usual gyrostatic effects.

I think then that the fallacy of the Machian view is that it mistakes a symptom for the disease. The danger of using the term absolute rotation is that it encourages this misunderstanding, because the term rotation refers to the symptoms. On the other hand, I think we cause graver misunderstanding if we deny the existence of absolute rotation. A practical illustration of this can be given. Many people have thought that the radiation from a moving star will, by exerting a back-pressure on the star, gradually reduce it to rest; this is clearly impossible, for there is no " rest " for it to be reduced to. On the other hand, Einstein has shown that a spinning rod radiates gravitational energy, and will ultimately come to rest; there is no fallacy in this, because the state here described as rest or " non-rotating " exists and is unique.

---

* Note that the disc being continuous (not atomic) there is no identification of individual parts of the disc ; and the velocity which we should ordinarily attribute to it is not a velocity capable of kinematical definition, but the quotient of momentum by mass.

The argument for retaining the term absolute rotation depends largely on the accidental circumstances of the actual universe. If we had to deal with a world containing very much greater masses in close proximity, we should find that (*a*) relative rotations of objects had no obvious connection with their absolute rotations, (*b*) much of what we were calling absolute rotation was more naturally regarded as a tidal effect of other bodies. While we should still recognise the vector Ω, there would be much less inducement to call it rotation. The whole question is one of nomenclature, and our preferences are likely to be different according as we regard it from the standpoint of actual observation or of a general formal theory.

THE FORMS OF CARBON AND CHEMICAL AFFINITY.

BY PROF. H. E. ARMSTRONG, PH.D., LL.D., D.SC., F.R.S.

THE ensuing discourse was written at the desire of some Mathematical Gentlemen—to adapt the form of prefatory words with which Sir Isaac Newton opens his *Opticks*—as a modern (perhaps, old-time) chemist's endeavour to aid in doing homage to the memory of a man whose genius touches the science of chemistry at every point, especially his affirmation, that the main business of natural science is to argue from phenomena without feigning hypotheses and to deduce causes from effects.

The balance of precision is the chemist's inseparable companion, as he constantly uses it in the estimation of mass, so that his thoughts are often turned, it must be supposed, at least subconsciously, to the operations of gravity. With the aid of the prism he has captured the world and is as certain of the external composition of the individual members of the stellar universe, at large, as he is of that of the insignificant unit in its vast system upon which he lives. With the prism also he is gradually guessing the intimate secrets of colour and even contemplates that he may be able, with its aid, to develop a justifiable interpretation of atomic structure.

Newton appears to have been an enthusiastic chemist—probably he had full command of the knowledge of the day. He was Master of the Mint and interested in the transmutation of metals—perhaps, as most were in his time, on the monetary side, not as an abstract proposition. The extent to which we are realising the alchemists' dream, though in no way foreshadowing pecuniary gain, would certainly have astonished him ; probably, however, he would have counselled a greater excess of caution in accepting results proclaimed in this field than that which now actuates the public mind, even that of the worker engaged in the quest. It is clear that he was a practised experimentalist. He even wrote upon the Nature of Acids. What most interested him, apparently, was the varying affinity displayed by what we, to-day, call the " radicles " of salts for one another. He was particularly concerned with phenomena such as are apparent when, for example, sulphuric acid is added to water, whereby much heat is liberated—in fact, he was a student of the phenomena of so-called *residual affinity*, which remain a mystery to us even to-day. Lastly, he may almost be classed as the father of organic chemistry, as he seems to have been the first to suggest that the diamond was combustible—he described it as an " unctuous substance coagulated," although, it must be admitted, upon evidence which is not very cogent. Tennant in 1797 burnt both diamond and charcoal carbon by means of nitre and, as each gave the same amount of carbonic acid, concluded that diamond is a form of carbon. Newton's test of "unctuousness" was largely optical and to-day we are using means—X rays—to discover the inner structure of the diamond and crystalline matters generally of the same order as those used by Newton. As refractive power in association with density formed the basis of his argument, he may also be said to have commenced the inquiry into optical properties, to which so much attention has been paid by chemists.

THE FORMS OF CARBON.

During the past three-quarters of a century, chemists have directed their efforts very largely to the study of the *character of carbon*, as displayed in the multitudinous forms in which it appears in organic nature or as products of laboratory and factory activity. The present opportunity is one, therefore, that may well be seized upon to give a broad survey of the position in which our knowledge of carbon stands to-day. It is clearly the foundation stone of life—no other element has the same plasticity nor the same trustworthy steadiness; no other element can give rise to so infinite a series of permutations and combinations. If there be life upon other worlds, it must be based, as here, upon carbon.

The diamond is practically pure carbon but the properties of the diamond cannot be said to be the properties of the element carbon: they are those of a complex system, of a system formed by the union of individual atoms. The mode of union can now be stated with great precision and it is worth noting here that the number of atoms ranged face to face in the diamond, along a length of a millimetre, is many millions—the distance between the centres of contiguous atoms being 1·54 Angström unit, the 1/10,000,000 of a millimetre.

Character cannot be inferred from mere inspection: it is only by studying their acts that we learn to know men. So it is with the elements. We have to study their acts at every stage and in every form, to appreciate their properties and the companionships in which they engage.

Paraffinoid Carbon. Carbon forms two simple compounds with oxygen—a monoxide and a dioxide, in symbols CO and CO_2. The primary hydride of carbon is *Methane* [marsh gas or fire damp], CH_4. As the single atom of oxygen combines with two atoms of hydrogen, forming *Hydrone*, OH_2, the unit component of the water complex, a molecule of methane is the equivalent of a molecule of carbon dioxide, in the sense that, in the former, four unit (univalent) atoms of hydrogen, in the latter, the equivalent two atoms of bivalent oxygen, are associated with the single quadrivalent atom of carbon. No compound of carbon is known in which a higher proportion than this of any other element is associated with its single atom. Although the superior valency of the carbon atom is thus four, carbon has apparently a multitude of inferior valencies, if only empirical composition be taken into account: the hydrides of carbon, indeed, are numberless, yet these are all to be reduced to certain simple types of union.

Methane is but the lowest term of a series. If an atom of hydrogen be withdrawn from each of two molecules, the residues coalesce

$$CH_4 - H = CH_3 \quad CH_3 + CH_3 = CH_3 . CH_3$$

The process may be continued, apparently to any desired extent: compounds are thus produced, such as

$$CH_3 . CH_2 . CH_3 \quad CH_3 . CH_2 . CH_2 . CH_3 \quad CH_3\,(CH_2)_x\,CH_3$$

in which the chain of carbon atoms is straightforward. Branched chains may also be constructed, such as

$$CH_2(CH_3)_2 \quad CH(CH_3)_3 \quad C(CH_3)_4$$

In all these hydrocarbons, the simple ratio preserved is expressed in the formula

$$C_nH_{2n+2}$$

The hydrocarbons are termed paraffins, because solid paraffin (so-called on account of its chemical indifference) is a mixture of higher terms of the series. Ordinary petrol is rich in the lower terms of the series. The hard natural fats are paraffin derivatives.

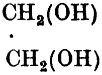
The paraffins are spoken of as *saturated* hydrocarbons, simply because they do not combine with other substances. A multitude of derivatives is obtainable from them, by displacing one or more atoms of hydrogen by other equivalent radicles, simple or compound, thus:

CH_3Cl CH_2Cl_2 $CHCl_3$ CCl_4

$CH_3(OH)$ $CH_3 . CH_2(OH)$ $CH_2(OH) . CH_2(OH)$

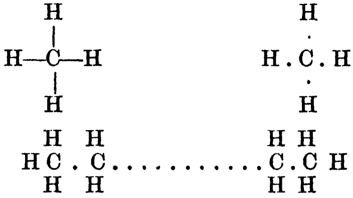
$CH_2(OH) . CH(OH) . CH_2(OH)$

$HCO(OH)$ $CH_3 . CO(OH)$



$$\begin{array}{c} CH_2(OH) \\ \cdot \\ CH_2(OH) \end{array}$$

Up to the year 1875, chemists were accustomed to write structural formulae in which the *valency* of the carbon atom was represented by attaching to it either four lines or dots, thus:



$$\begin{array}{c} H \\ | \\ H-C-H \\ | \\ H \end{array} \qquad \begin{array}{c} H \\ \cdot \\ H . C . H \\ \cdot \\ H \end{array}$$

$$\begin{array}{ccccccccc} & H & & H & & H & & H & \\ H & C & . & C & \ldots\ldots\ldots\ldots & C & . & C & H \\ & H & & H & & H & & H & \end{array}$$

The complex paraffinoid compounds were regarded as long straight chains, in which the individual links were two terminal groups of the CH_3 form and each of the intermediate links the complex CH_2. To judge from experience, such chains are of uniform strength throughout: no increase in the number of links seems to alter the strength of individual links. The affinity for hydrogen and for itself exercised by the carbon atom in the paraffin, in fact, would appear to be practically constant. As the series is ascended, gases gave way to liquids and these to solids. As the paraffins are saturated compounds, it may be assumed that carbon and hydrogen are mutually satisfied in these compounds—that neither has *marked* residual affinity.

Ethenoid Carbon.—The paraffins are easily deprived of hydrogen. Many methods are known by which the withdrawal may be regulated. To give an example, ordinary alcohol is easily deprived of the elements of a molecule of hydrone and converted into ethylene:

$$C_2H_6O - H_2O = C_2H_4$$

Ethylene is unsaturated: it readily combines with bromine:

$$C_2H_4 + Br_2 = C_2H_4Br_2$$

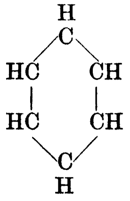
The product, it will be observed, is paraffinoid—*i.e.* of the form C_2H_{2n+2}. Ballistic evidence has been obtained by Sir J. J. Thomson and Dr. Aston of the ephemeral existence of radicles formed by the withdrawal of a single atom of hydrogen from the paraffin; when withdrawn from the dissociating influence of the electric discharge, such molecules coalesce to form higher paraffins. The belief was long held that compounds containing a carbon atom with a single free affinity could not exist as free molecules. This is generally true but exceptions to this rule, of remarkable interest, are to hand in the triphenylmethane series. *Triphenylmethane*, $CH(C_6H_5)_3$, for example, may be divested of its hydrogen atom, forming *triphenylmethyl*, which is stable as a pure substance though it is easily attacked by oxygen and other agents.

Ethylene (ethene) is the first term of a series of hydrocarbons of the form C_nH_{2n}. The lowest conceivable term of the series, methylene, CH_2, has only an ephemeral existence, like the radicle CH_3 but unlike carbon monoxide, CO.

The product of the changes in which it might be formed is usually ethylene, $C_2H_4 = 2CH_2$. Chemists have long disputed over the manner in which the two carbon atoms in ethylene are united. Obviously, if they were united, twice over, by two full affinities of each, as the atoms are singly in $H_3C \cdot CH_3$, ethylene and ethenoids generally would be saturated. As it is, they are all *unsaturated*: either there is no second union or the "doublet" is formed in some peculiar way. The writer, on this account, has always used a special symbol, in teaching, to represent the ethenoid union, viz. H_2COCH_2, two curved lines, instead of two straight lines—leaving it open what the precise form of union is.

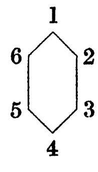
Acetenoid Carbon. It is possible to withdraw three units of hydrogen from methane, leaving the radicle CH: this is even less stable than methene, CH_2; whenever formed, it "doubles" into acetylene, C_2H_2, now well known on account of the wide use that is made of it as an illuminant. Acetylene is eminently unsaturated, being raised, with the greatest ease, by combination with bromine, acids, etc., to compounds of ethenoid type and then to the paraffinoid type. The manner in which the two carbon atoms are united is more obscure even than in ethylene: it is certainly open to question whether there be any form of triple linkage, even of an inferior kind, comparable with that in ethylene, between the carbon atoms.

Phenoid Carbon. The paraffins and the parallel series of ethenes and acetenenes are simple (open) chains, the terminal "links" being free. The great majority of known hydrocarbons are of another type—*closed* systems. Benzene, now so largely used as motor fuel, is the root example of this class. The molecule contains six carbon and six hydrogen atoms, as expressed in the formula C_6H_6. This differs from that of the corresponding paraffin hexane, C_6H_{14}, by H_8. Not only, however, does it never combine with 8 units of another radicle but its whole behaviour is that of a saturated compound; indeed it gives rise to substitution derivatives with peculiar ease. When, under suitable special conditions, it is combined with chlorine, it accepts only six and never eight atoms. This last behaviour has led to the conclusion that the six atoms of carbon are not only linked, successively, the one to the other but that the ends of the chain are joined together. Chemists have long been accustomed to speak of the *Benzene Ring* and as benzene is characterised by a remarkable symmetry in its general behaviour the ring is represented by the formula subjoined:

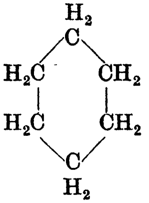


More frequently a simple hexagon is used, numbered in the manner shown, so that the relative positions taken up by radicles introduced into the complex in place of hydrogen atoms may be indicated:

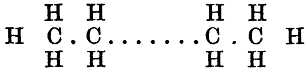
The manner in which the fourth affinity of each carbon atom in benzene is disposed of has given rise to endless discussion. It may be mentioned that the names Phéne (*Anglice,* Phene) and Benzéne were proposed in 1843 by the French chemist Laurent, the former on account of the high refractive power of the hydrocarbon.



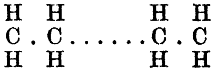
Phanoid Carbon. It is possible to combine benzene with six hydrogen atoms and convert it into so-called hexamethylene or hexhydrobenzene. This hydrocarbon is scarcely distinguished from the paraffin hexane and being formed from phene may well be called hexaphane, the terminal *ane* being used to indicate *saturation.*



Polymethylenic Carbon. Obviously, if a hydrogen atom could be merely removed from each of the two ends of a paraffin, a polymethylenic, simple chain would be formed, thus :



$$\begin{array}{ccccccc} & H & H & & H & H & \\ H & C\,. & C & \ldots\ldots\ldots & C\,. & C & H \\ & H & H & & H & H & \end{array}$$



$$\begin{array}{ccccc} H & H & & H & H \\ C\,. & C & \ldots\ldots & C\,. & C \\ H & H & & H & H \end{array}$$

Actually, it is possible to prepare a trimethylene, $(CH_2)_3$, by withdrawing the bromine from the bromide, $BrCH_2 . CH_2 . CH_2Br$. There are two schools of thought, one holding that the properties of the product are such as to justify the assumption that the ends of the chain remain uncoupled, the other that they become joined, as in hexaphane, forming a " ring." This latter hypothesis has been applied exclusively to the polymethylenic compounds above trimethylene containing four and five atoms of carbon.

The Carbon Atom. Chemical experience tends to show that, whatever the nature of affinity may be, the four affinities of the carbon atom are symmetrically exercised and equal. This conclusion is embodied in a solid model, if a regular tetrahedron be taken as representative of the carbon atom. Although this conception is implicit in Pasteur's earliest work on the tartaric acids and had been in other chemists' minds, it was first brought into operation by the distinguished Dutch chemist, van 't Hoff, in 1874. Van 't Hoff specially drew attention to the fact that, if four different radicles were attached to the tetrahedron, an asymmetric system was produced, of which there were two non-superposable forms, one right, the other left-handed in arrangement—corresponding to Pasteur's two isomeric tartaric acids, deflecting rays of plane polarised light equally and oppositely. The success with which this hypothesis was applied to the study of optical activity was such that chemists were carried away even by the limited use that was made of it by van 't Hoff.

The conception he developed was, that the four affinities in the carbon atom acted from the centre of mass towards the apices—consequently at the tetrahedral angle 109° 28′. He pictured *paraffinoid* compounds by means of tetrahedral models so joined. To represent the *ethenoid* mode of union, he joined two tetrahedra edge to edge ; to represent the *acetenoid* mode of union, he joined them face to face. When ethenoid compounds were so represented, differences became obvious, depending upon the relative positions occupied by radicles attached to the four apices of the twin carbon systems—here again practice coincided with theory.

Obviously, the method of union pictured by van 't Hoff involved a distortion, from that normal to the single atom, of the directions in which affinity acted. The recognition of this consequence, in fact, led to the development of a strain hypothesis by von Baeyer, who pictured the affinities as departing from the normal angle by 24° 44′, 9° 34′, 0° 44′ or $-5°$ 16′, according as the ring was constructed of 3, 4, 5 or 6 atoms of carbon. As a matter of fact, the behaviour of the compounds considered was not such as fully to justify this conception.

The tetrahedral conception has been entirely justified by Sir William Bragg's masterly X-ray study of Sir Isaac Newton's " unctuous substance," coagulated

Diamond. This is shown to consist of atoms united at the tetrahedral angle in "hexaphane" arrangement, as though it were built up of an endless succession of six-atomed systems, each having the arrangement which has long been assigned to benzene. Each carbon atom is surrounded with and equally attached to four other like atoms. Hence the hardness of diamond. Only the atoms at the surface are in any way different—each of these must be supposed to have a free affinity projecting outwards, so that the surface is unsaturated.

The tetrahedral hypothesis has been further justified by the results obtained by Sir William Bragg and his school from an examination of the paraffin hydrocarbons and related fatty acids, etc. The carbon atoms are recognisably united in long zig-zags, each atom being united to the next at the tetrahedral angle. In the solid, the zig-zags are arranged side by side, interlocked in the direction of their length, the length being the multiple of the distance between the centres of contiguous carbon atoms (1·54 A.U.) multiplied by the number of atoms.

The results obtained by Sir William Bragg have been confirmed by the measurement of the space occupied on water by thin films of the fatty acids and other paraffinoids which we owe especially to Langmuir and Adam. The acid becomes spread out in a layer a single molecule in thickness, in which the molecules are arranged side by side, upright like scaffold poles, the carboxyl radicle (CO . OH), which is characteristic of acids, resting upon the water at this tip.

Solid Geometry of Carbon Compounds. Although the hypothesis developed by van 't Hoff was based upon the tetrahedron, neither he nor those who have followed him have made any logical use of the conception in constructing models to represent structure. Owing to the general disregard of crystallography by chemists, except as a means of identification, the attempt to correlate structure with crystalline form has failed thus far to engage their attention. Up to the time when X-rays were used for this purpose, the only workers in the field were Pope and Barlow, whose first attempt was published in 1899.

The fundamental postulate underlying their treatment of the problem was one connecting volume directly with valency. They assumed that valency was a function of volume. Representing univalent hydrogen by a sphere of unit volume, bivalent oxygen, tervalent nitrogen and quadrivalent carbon were represented respectively by spheres of twice, thrice and four times the volume of the hydrogen unit sphere. They closely packed such spheres, in the order indicated by chemical considerations, to represent molecular structure. Assuming such models to be squeezed down until the intervening spaces were filled, they calculated the corresponding crystallographic constants. To obtain results in fair harmony with practice, corrections were necessary which, in the end, could not be justified. Still, an important beginning had been made: the chapter was opened. I had long been systematically accumulating crystallographic data, in the hope of dealing with the problem of molecular structure of solids. With the aid of Messrs. Colgate and Rodd, the Pope-Barlow method was applied to these, seemingly with success. We have now to recognise that our attempt was premature.

About a dozen years ago, approaching the problem separately from the two sides of geometrician and chemist, Mr. Barlow and I modified the method of treatment, by making models of unit spheres, using one, two, three or four of these to represent hydrogen, oxygen, nitrogen and carbon. The model of carbon then became a tetrahedral pyramid of four spheres. Assuming the spheres to be squeezed down into close packed contact, the model became a regular dodecahedron and the model of the carbon atom a tetrahedral figure consisting of four such dodecahedra.

Such a model embodies the fundamental properties of the carbon atom very closely, especially the equality and direction of the four affinities, each of which may be supposed to be located at the centre of a face (terface) at which three dodecahedra are exposed.

The consequences indicated by such a model are remarkable and far reaching.

The paraffinoid union is effected by interlocking two models at their terfaces: they are thereby fixed rigidly and cannot rotate so long as they remain in close contact. The models may be packed together, to any desired extent, in this manner. To form diamond, the models are arranged side by side, alternately base downwards and base upwards, sheets thus formed being piled one upon the other: throughout such a mass, each unit is similarly placed with reference to its four neighbours.

There is but one other alternative way of uniting and close-packing the carbon models: that of interlocking two tetrahedra jaw-wise, as it were, so that two dodecahedra of the one grip two of the other, instead of three being interlocked with three. It is tempting to regard this as the model of ethenoid carbon: the more as chemists are inclined to think that far from being more firmly held together than in the paraffinoids, ethenoid compounds are less firmly united. This kind of union is, moreover, in a sense *a double bond*, as two single affinities of each atom are concerned and spoiled, as it were; each atom having but two paraffinoid surfaces free. It is impossible, using such a model, to represent two carbon atoms as, in any way, united by, at or through three affinities: it seems necessary, therefore, to assume that the carbon atoms in acetylene may be united merely as in ethylene, leaving each carbon atom with an affinity free.

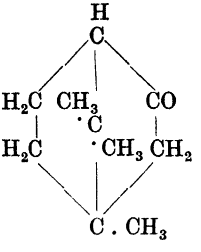
Stated briefly, if such models be used in constructing *close-packed assemblages* of carbon atoms to represent carbon compounds generally, only two forms of union can be represented.

The arrangement in benzene, C_6H_6, is that in the diamond: benzene appears to be but a hexagonal piece, cut out of a single sheet of " diamond " atoms, around the periphery of which are symmetrically placed six unit dodecahedra, representing the six atoms of hydrogen. The six carbons form *a closed system*, in which the contiguous units are all held together in unstressed paraffinoid union and united at the tetrahedral angle. The six hydrogens are arranged symmetrically in the two planes of the carbon model—three in the upper, in the relative positions 1 : 3 : 5, three in the lower, in the relative positions 2 : 4 : 6. Three free " paraffinoid " affinities appear at each of the " free " surfaces of the model. Assuming these to be collected into two bundles which are in balance, the behaviour of benzene is such as to indicate that these residual affinities in no way have the full force of paraffin affinities. The association of six atoms of hydrogen with the hexagonal complex of carbon atoms appears, in some way, to have the effect, as it were, of pulling down the force of affinity into the mass and weakening its display at the free surfaces. What actually happens we cannot say at present and it is useless to speculate. We can only await the time when the theory of atomic structure will be sufficiently advanced to permit of our discussing the meaning of " planetary perturbations " and dynamic peculiarities such as these with advantage.

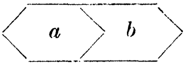
The use of the tetrahedral carbon model imposes some restrictions. *No less a number than six can be arranged in a closed system.* Moreover, as the angle at which the models meet, in a close-packed arrangement, is invariable, the practice long favoured by chemists of representing tri, tetra and penta-methylene compounds as closed systems is more than open to question. To discuss this issue here is impossible: all that can be said is that the chemical evidence upon which such conclusions have been based is in no way satisfactory or sufficient. The subject needs thorough revision in the laboratory.

Thus far, chemists have been too much inclined to think in terms of plane formulae drawn only in two dimensions upon a paper surface: they have

yet to think in terms of the solid, to make real and full use of the van 't Hoff tetrahedron. For example, it is easy to represent benzene by a ring formula and, on paper, to represent atoms as filled into the ring, as it were, bridging it in the positions 1 : 4. Camphor is an example, as shown in the conventional formula



When built of tetrahedral models, however, the C_6 complex is not only compact and solid but the paraffinoid affinities of the atoms 1 and 4 " reach out " into different planes. It is only possible to bridge positions 1 : 3. The following conceivable skeleton representation of the carbon system of camphor, in point of fact, is more in accordance with the general behaviour of the substance:



In the solid model, it is seen to consist of two fused " hexaphane " systems, with one-half in common, section *b* being in a plane highly inclined to *a*.

Taking the model of benzene described above as nucleus, Mr. Barlow has been able to correlate crystalline form and structure in a considerable number of benzene derivatives, the difference between the values deduced from the model and the crystallographic values being only about 2-4 per cent.

In most cases the derivative is formed by the mere apposition to the model of the appropriate radicle in place of one or more hydrogen atoms. In certain compounds a change of a more or less drastic character must be made to secure close-packing of the molecular units. Parabromphenol and triphenylmethane are cases in point. To secure a fit and make the model accord with crystallographic peculiarities, it is necessary slightly to shift several of the hydrogen units and yet only to vary the position of the unit upon the carbon atom to which it naturally belongs from an upper into a lower plane or *vice versa*. In all cases the carbon complex remains unchanged.

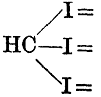
Nothing short of a new field of speculation and inquiry is opened out by the introduction of the geometrical method of study which these models involve : they seem, in fact, to afford a definite means, of an altogether novel character, of exploring solid structure. In the X-ray method, little notice can be taken of hydrogen—at the most, it affords positions of weakness which serve as lines of demarcation, as reflexion only takes place from the centres of atoms of greater mass than that of hydrogen.

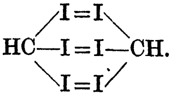
The question of interest for future consideration is, whether displacements such as those indicated may not be in part or entirely consequent upon the formation of crystals : it may well be, that in the single, separate, free molecule, the peripheral units usually occupy normal positions and that the displacements are effected only or in part when residual affinity comes into play between the molecules in their passage into the crystalline state.

THE ORIGIN OF SUBSTANTIVE COLOUR.

What we call Colour is an affection of the eye. The colour we see is always a residual effect, compounded of the vibrations remaining after the withdrawal of some of the wave-lengths from the totality of those which give rise to the complete visible spectrum. The effect is produced in two ways : either " physically "—through the agency of thin plates or films or fine particles, by diffraction—or " substantively " by means of definite, light-absorbing materials. Chemically speaking, these latter may all be broadly classed together : it is possible to recognise one particular type of structure in them all.

The elementary materials, chlorine, Cl_2, bromine, Br_2, and iodine, I_2, are all coloured. Most of their compounds, however, are colourless : it is probable that, as *monatomic molecules*, they would be colourless. Mercuric iodide, HgI_2, is colourless in certain solvents but yellow to orange-red in the solid state : the solid chloride and bromide of mercury are white. It is probably not going too far to ascribe colour in the iodide to the formation of molecular complexes in which several iodine atoms are conjoined to form light-absorbing centres.

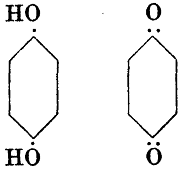
A more striking illustration is that afforded by the iodine derivatives of methane. Iodomethane, CH_3I, is a highly refractive colourless liquid. Diodomethane, CH_2I_2, though still more refractive, is also colourless. Both are what Sir Isaac Newton termed " unctuous substances." Triodomethane (iodoform), CHI_3, is a yellow crystalline solid. Tetriodomethane, CI_4, is dark ruby red. Chlorine, bromine and iodine are normally univalent elements but, on occasions, they manifest a higher valency, iodine especially often that of a triad. The colour in iodoform is, therefore, conceivably due to the co-operative light-absorbing effect of the three contiguous unsatisfied centres (valencies) of the three iodine atoms. Maybe, the simple fundamental molecule  is colourless and the coloured form is a complex,



This is a problem yet to be solved.

Iodoform may be taken as the type of coloured carbon compounds generally : with few exceptions yet to be unravelled all of these appear to be coloured, because of the presence of one or more such triple absorbing centres. When only one such set is present, usually only short wave-lengths are absorbed and colour may vary from yellow to red. In a few instances, however, the colour is blue or green : in these latter, the absorbing centres are all different. When several sets of such absorbing triplets are associated, the colour is never a pure yellow or red but is increased in intensity and there is great variation in its character. When the absorbing mechanism is sufficiently complex, the colour verges on black.

The typical coloured carbon compound is *Quinone*, the substance obtained by withdrawing two atoms of hydrogen from the colourless dihydroxybenzene quinol (hydroquinone).



Quinone is believed to contain two atoms of oxygen *in ethenoid linkage* with the benzene residue C_6H_4, as shown by the above formula. These two incompletely saturated ethenoid linkages, in conjunction with the centre afforded by the benzene residue, appear to act as the absorbing triplet. As the great majority of coloured compounds—thousands are known—may be referred to the quinone type, the hypothesis here developed is known as the *Quinonoid* hypothesis of colour. Thus far, no other comprehensive explanation of the phenomena has been suggested.

The probability is that colour not infrequently arises or at least is increased in intensity through the formation of extra (inter) molecular-absorbing systems —as suggested in the case of iodoform and mercuric iodide.

When the Barlow models of quinone are close-packed and stacked one upon the other, not only are the units brought into apposition at their oxygen centres but these also fall one upon the other, constituting regions which may be supposed to be specially absorptive.

To sum up—colour appears to be the outcome of an effect exercised at absorbing centres of an ethenoid character. It is an ethenoid function: a function of residual affinity—the force by which molecules generally are constrained into the liquid and solid condition.

Black Carbon. In addition to the diamond form of carbon, it is customary to recognise two other forms—the one being that of *Graphite*, which is crystalline, the other that of the various charcoals or chars. I have long been heretic enough to doubt whether there be more than one form of Newton's "unctuous substance coagulated." In chemical behaviour the two materials are peculiar. Graphite, on oxidation, yields a crystalline acid, graphitic acid, from which, it is said, benzenehexacarboxylic acid, $C_6(CO.OH)_6$, may be obtained. The chars also yield this acid and several simpler acids derived from benzene. Both, therefore, are in part benzenoid in structure. This view has been confirmed by X-ray analysis. It has not yet been shown, however, that either graphite or the chars consist of carbon pure and simple. Considering the structure of the diamond, the even manner in which affinity appears to be exerted within its mass, any other form of structure probably would be less stable and tend to pass over into diamond. As a matter of fact, the conversion of diamond into graphite is easily realised (in presence of moisture) but never the reverse change. There is reason to think that a minute atomic proportion of hydrogen may be present, sufficient to make the existence of the benzenoid arrangement possible and stable. To explain their blackness, in the light of the colour hypothesis sketched above, it is necessary to assume that both graphite and the chars are largely ethenoid as well as benzenoid. It is conceivable that sheets of carbon atoms in benzenoid arrangement are superposed upon sheets in which the ethenoid and paraffinoid forms are associated. The argument thus briefly stated will be sufficient to show, that the problem the structure of "carbon" presents is one that can only be settled by chemist and physicist acting in co-operation. At present, there is a tendency to overrate the powers of the newly introduced X-ray method of studying structure and to treat its results as absolute: in point of fact, they are but of relative value, involving as they do a vast amount of "interpretation."

DE NATURA ACIDORUM.

Sir David Brewster specially refers to Newton's interest in the subject of acids.

The third book of the *Opticks* (4th edition) is full of experimental observations and reflexions upon chemical interactions. In Question 31 he asks:

"Have not the small particles of bodies certain powers, virtues or forces, by which they act at a distance, not only upon the rays of light for reflecting, refracting and inflecting them, but also upon one another for producing a great part of the phenomena of Nature? . . .

"The Attractions of Gravity, Magnetism and Electricity reach to very sensible distances and so have been observed by vulgar eyes, and there may be others which reach to small distances as hitherto escape observation; and perhaps electrical attraction may reach to such small distances, even without being excited by friction. . . .

"For when salt of tartar runs *per Deliquium*, is not this done by an attraction between the particles of the salt of tartar and the particles of the water which float in the air in the form of vapours? . . .

"And is it not from the like attractive power between the particles of Oil of Vitriol and the particles of water, that Oil of Vitriol draws to it a good quantity of Water out of the air, and after it is satiated draws no more and in distillation lets go the water very difficultly ? And when water and Oil of Vitriol poured successively into the same vessel grow very hot in the mixing, does not this heat argue a great motion in the parts of the liquors ? And does not this motion argue that the parts of the two liquors in mixing coalesce with violence, and by consequence rush towards one another with an accelerated motion ? "

Newton is here but considering the operation of the force of residual affinity, which is not only unexplained to the present day but rarely considered. In his subsequent discussion of the interactions of acids and metals and of acids and salts, he is dealing with the subject of ordinary chemical change. Speculation on these subjects to-day is not less vague than it was in Newton's time. Modifying his language but slightly, to reduce it to modern terms, we can but realise that little advance has been made in our understanding of the phenomena—although we have an exact and abounding knowledge of fact which is astounding compared with that of Newton's day. We need a Newton with the perspicacity to order our knowledge into a philosophy.

Take the following all but perfect picture :

"For the air abounds with acid vapours fit to provide fermentations, as appears by the rusting of iron and copper in it, the kindling of fire by blowing and the beating of the heart by means of respiration. Now the above-mentioned motions are so great and violent as to shew that in fermentations the particles of bodies which almost rest are put into new motions by a very potent principle, which acts upon them only when they approach one another and causes them to meet and clash with great violence and grow hot with the motion and dash one another into pieces and vanish into air and vapour and flame."

Newton here anticipates Lavoisier, who, at the close of the century, early in which this passage was written, was to be inspired to rechristen the " Acid Vapours " spoken of by Newton, as present in the air, which Scheele, Priestley and he had isolated and characterised, to hide Newton's meaning in the Greek word *Oxygen*. The Teutonic world alone had the good sense to retain *Sauerstoff*, a word that may be understood by all who either hear or see it, which we might well use. Oxygen, to classic eyes and to our present Greekless eyes, has no understood meaning. To-day, we have reduced the conception " Acid " to the most miserable of dimensions—to a figment of the imagination, the lonely hydrogen ion. Verily, is our ignorance far greater than that of Newton, near two hundred years ago, because, having eyes to see and a vast mass of recorded fact to consider, we will take no notice of fact and are but worshippers of faiths which have no secure basis ? To pursue the subject here is impossible.

Whether we study diamond, whether we study colour, whether we study acids or other characters, we are always faced with this one problem, that which struck Newton's attention, the problem of chemical affinity. We now believe it to be electrical in its origin. Further it is impossible to go at present—the speculation thus far indulged in seems trivial in face of the complexities the subject presents. We are told that the electron does everything but *how* it does anything we are not informed.

We need, indeed, to follow Newton's advice—to argue from phenomena with less feigning of hypotheses and to deduce causes more from effects. Never was superstition more rife in scientific circles than it is to-day—the apostolic injunction, " Prove all things, hold fast that which is true," is not practised, at least among chemists and physicists. Above all, we need to clear our minds by argument, if there be any left to argue, with reasonable attention to the facts.

I trust that these simple reflexions may not be unworthy of the occasion on which they are offered.

TWO UNPUBLISHED DOCUMENTS OF SIR ISAAC NEWTON

By Prof. David Eugene Smith, Ph.D., LL.D.

The following documents, here published for the first time, are mathematical to only a slight degree. They show, however, something of the little-known side of Newton's mind—first as it appeared when he was a boy or a young man, and later as it showed itself when he was approaching old age, in the period when he was Master of the Mint.

In the memoirs of Newton (vol. i. p. 17), Sir David Brewster tells us that Conduit, Newton's nephew, mentions in his notes two memorandum books of his uncle, saying that the first of these " to which he has put his name, and 1659," contained " Rules for drawing and making colours," the other being of the same year and containing " *Prosodia* written out." Brewster adds: " The *first* of these books I did not find among the family papers ; but the *second* is the one referred to in the text."

The first of these notebooks has for a long time been in the Pierpont Morgan Library, New York, where there are also ten other documents in Newton's hand, all these latter pieces having been written during the later years of his life. The book is about 2¾ by 4⅞ inches in size, and on the first page there is the inscription :

Isacus Newton hunc librum
possidet.
teste
Edvardo Secker.
pret : 2^{d} ob.
1659.

The document shows by its handwriting three periods of composition. The first style is characterised by the rather precise hand of one who was still following the copybook ; the second shows the same general form of letters, but the writing gives evidence of less deliberation ; the third style is more mature, and is that with which Newton scholars are more familiar. The forms of the letters in all three periods, however, show the Newton characteristic capitals of such letters as *A*, *C*, *P*, *M*, *N*, and *T* ; the *o*-shaped *e* which is so familiar in his signature ; and his aversion to crossing the *t* by a separate line.

By the kindness of the Trustees of the library, permission has been given to publish in this anniversary number of the *Gazette* two Newton documents : (1) The essential features of this notebook, and (2) Newton's proposal for the apprehension of counterfeiters, made when he was Master of the Mint. The first represents him both as a boy and (if we may infer properly from the handwriting and the nature of the text) as a student in his first year at the university. The second represents him after his retirement from Cambridge and from what may be looked upon as his real mathematical life.

The notebook consists of three general parts : (1) A list of directions for drawing ; of recipes apparently from some country physician, and of simple chemical tricks; and of a scheme for reformed spelling; (2) a word list consisting of a classification of topics under a partial scheme of knowledge; (3) astronomical tables and two solutions of triangles. Judging by the writing and the maturity shown, the first seems to belong to the period of about 1655-1658 (age 13-16); the second, to about the year 1659; and the third probably to his first year at Cambridge (1661-1662), a belief strengthened by the date of the first year (1662) of the calendar which he computed for a future period of twenty-eight years. The second part has the general caption, " The several things contained under those general heads." The subheads are: (1) " Artes, Trades, & Sciences "; (2) " Birdes, Chap. 2 vide Nomencleturam, pag: 37 "; (3) " Beasts: Chap. 3: vide Nomencleturam pag: 39 "; (4) " Cloathes. Chap. 4 "; (5) " Of a Church. Chap. 5 "; (6) " Of Diseases. Chap. 6 "; (7) " Of the Elements. Chap. 7 "; (8) " Of ffishes. Chap: 8 vide Nomencleturam. pag 43 "; (9) " of Hearbs & Woodes. & fflowers. Chap: 9."; (10) " Of a House & Housaldstuffe. Chap: 10."; (11) " Of^{e} Husbandry Chap 11."; (12) " Instruments & things belonging to Artes. Chap: 12."; (13) " Of Kindreds, & Titles. Chap. 13 "; (14) " Of Man, his Affections & sences. Chap. 14 "; (15) " Of Meate & Drinke Chap: 15 "; (16) " Of Mineralls. Chap: 16." This list consists of forty-two pages, and under each of the sixteen titles the words are arranged alphabetically by the first letter. There is no statement as to whether or not they referred to some book which Newton had been reading or were designed to represent some scheme of knowledge that he had worked out in another manuscript.

Besides the name on the first page there are two places in which the initials " J.N." appear, the " J " being more nearly in print form than in his full signature. These are in the early or middle period of the text.

TEXT OF THE FIRST PART.

Of Drawing. [This running headline is continued for ten pages.]

Instruments of drawing.

Pens made of Raven quils. thick & smooth paper & light coulored blew pap^{r}. fine parchment. a flat thin bras ruler. a paire of compasses. a wing. & sundry plummetts & pestells to draw with-all.

Of plummets.

There are naturall plummets not made. as blacklead, black chalk. charecole. split red stone. white chalke. or there are artificall plummits.
Thus made.
Take a great chalk stone & make furrows in it 2 or 3 inches long & so wide as you may lay a quil in each of them. Then take a proportion of white chalke ground verry fine, temper it wth aile, or wort & a little new milke, & soe make pap thereof: then power it into y^{e} furrows of y^{e} chalke. you may soone tak them out & role them up, or let y^{m} lie in y^{e} chalk till they be quite dry. & then take them out & strane [?] y^{m} into a hansome forme. You may temper Lake wth burnt Alabaster for a red &
Alabaster burnt & bite [?] for a blew. & so for others. regarding some colors y^{t} will bind over hard w^{ch} must have a little water put on them in their grinding.

Of Drawing with y^{e} pen.

Let y^{e} thing w^{ch} you intend to draw stand before you, so y^{e} light be not hindered from falling upon it. & with a pointed piece of charecole draw it rudely & lightly. when you have don see if it be well don; if not wipe out wth yo^{r} wing & begin agine, & so draw it till it bee well. y^{n} wipe it over gently wth yo^{r} wing, so y^{t} you may perceiv yo^{r} former strokes; y^{n} wth yo^{r} black chalk or pensill draw it perfectly & curiously as you can, & shaddow it as y^{e} light falleth upon it. If you draw on blew paper when you have finished yo^{r} draught wet yo^{r} paper in fair wait & let it dry of it self. & so y^{e} drawing will hold fast on.

Of Drawing

Draw y^{e} utmost lines of yo^{r} garment & y^{e} greater folds first w^{ch} continue through y^{e} whole garment, y^{n} break y^{e} greater folds into lesse, & so shaddow them.

Of Landskip.

If you express y^{e} sunn make it riseing or setting behind some hill; but never express y^{e} moon or starrs but up on necessity.

Of Emblem or Empresse work.

In drawing after y^{e} life sit not nearer y^{n} two yards from y^{e} partie, & sit of one height. but if y^{e} party you draw be very tall let him sit above you a little if low or a child let him sit a little below y^{u}. If you draw from y^{e} head to y^{e} foot let y^{r} party stand at y^{e} least six yards frõ you. let y^{e} party stand for few can sit upright as they can stand.

ffirst draw y^{e} stroaks for y^{e} forehead w^{ch} must be done exactly, because according to y^{t} proportion must all y^{e} rest bee drawn as if y^{e} fore head bee soo long, y^{n} must it be twice so long from y^{e} forehead to y^{e} chin. then draw y^{e} farthest eye making y^{e} circle of y^{e} sight perfectly round & placing y^{e} reflection of y^{e} sight w^{ch} appeareth as a white spott, acording to y^{e} light. 3. drav y^{e} nose. fourthly y^{e} nearest eye, leaving y^{e} just length of an eye betweene it & y^{e} other. 5. Draw y^{e} mouth. 6 The chin. 7 finish y^{e} out line of y^{e} face. & lastly y^{e} hairs. having finished y^{e} head draw y^{e} whole body proportionable thereunto.

Of shaddowing.

To shaddow sweetly & roundly wth all is a far greater cunning y^{n} to shaddow hard & darke; for it best to shaddow as if it were not shaddowed.

To take a perfect drawgh of a picture. Take a sheet of Venice or of y^{e} first paper you can get, wet it all over wth cleane salitt oyle, y^{n} wipe y^{e} sallot away so y^{t} y^{e} paper may be dry throughly. then lay y^{e} paper on y^{e} picture, & you shall see y^{e} pitture through y^{e} paper, & then wth a pensill draw it over, & y^{n} wth a pen. Then take of y^{e} oyled paper & lay it on a cleane sheete, & wth a stick pointed, or a fether of a swallow wing, draw it over againe, & you shall have it neatly drawne on y^{e} white paper.

Another way.

Take some Lake and grind it fine & temper it wth linseed oyle, & then wth a pen & this mixture draw out all y^{e} greater strokes of any picture & also y^{e} muscles. Wet y^{n} y^{e} contrary side of y^{e} picture, & press it hard upon y^{e} sheet of cleane paper & it will leave behind it all y^{e} stroks you draw.

An easy way to lesson or enlarge a picture.

Make a square, & then divide it into divers equall parts with y^{e} compasses, & draw owethwart lines wth a ruler & a pensill, so y^{t} y^{e} pictur be divided wth equall squares, & so make squares on a faire paper as little or big as you will, but let ther bee soomany as there is in y^{e} picture, then observing y^{e} order of y^{e} squares draw y^{e} picture over wth a pensill pasing from square to square.

How to make Allum water

Take a quart of water & boyle it in a quarter of a pound of Allum, seeth it untill it be molten, & let it stand a day. wth this water wet over y^{e} pictures y^{t} you intend to colour for it will keepe y^{e} colours for sinking into y^{e} paper, & make them shew fairer, & continue y^{e} longer wth out faiding. You must let y^{e} paper dry of it selfe before you lay yo^{r} colours on it or wet it againe for some paper needs four or 5 wetings.

How to make gum water

Take cleane water & put gum Arabic a little into it, let it stand till y^{e} gum be dissolved, & let it not bee too thick for you cannot work well wth it. nor too thin for it will not bind fast enough. wth this water temper yo^{r} colours befor you lay them on.

To make lime water.

Take unslackt lime & cover it with water, an inch thick let it stand a night & power of y^{e} cleare water in y^{e} morning, & keepe it in a cleane thing for yo^{r} use. wth this water you must temper yo^{r} sap greens when you would have a blew coulour of it.

To mak water of sope ashes

Steepe sope ashes a night in raine water, in y^{e} morning power of y^{e} clearest. this is to temp^{r} yo^{r} brasill wth.

How to prepare yo^{r} colours.

Such colours as have need of grinding you must grind them wth faire water, y^{n} put them on a smoth chalk stone, & let y^{m} dry, y^{n} grind y^{m} againe wth Gum water & reserve y^{m} in muscle shels for yo^{r} use.

A sea colour.

Take privet berries when y^{e} sun entreth into Libra, about y^{e} 13th of September, dry y^{m} in y^{e} sunn ; then bruise them & steepe y^{m} in Allum water, & straine y^{m} into an earthen poringer y^{t} is glazed

Another.

Take blew Inde & steepe it in water & put to it a little Verditer.

A yellow colour.

Take yellow berries, & bruse y^{m} & steepe y^{m} a quarter of an hower in allum water y^{n} strain y^{m} if you will or let y^{m} stand in liquour.

A Haire colour.

Take umber or spanish browne grind it & temper it wth gum water.

A russet colour.

Take y^{e} fattest sut you can get & put it into a pot of cleare water so y^{t} be covered two or 3 fingers & let it seeth well w^{ch} don straine it through a cloth & set it on y^{e} fire againe to thiken (but take heed you set it not on too hot a fire for feare of burning it) & so let it boyle gently till it bee as thick as you would have it.

A colour for faces

Lay on y^{e} cheekes little spotts of lake or red lead y^{n} come all over it with white, & a little lake, shaddow it wth Lamblack or umber, & white lead.

Colours for naked pictures

Take white leade & a little Vermilion temper them & lay them on ; shaddow it wth bolearmenith in y^{e} middle & adde a little sut to y^{e} utmost double hatches [hatched ?].

A Colour for dead corpes.

Change white leade wth y^{e} water of yellow berrys & wash y^{e} picture all ouer y^{n} chang it wth blew Indie & shaddow it in y^{e} single hatches, & leanest places y^{n} take sut, yellow berrys & white lead & wth y^{t} shadow y^{e} darkest places.

A blood red colour.

Sinaper, lake, & vermillion make a good red.

Another

Take some of y^e^ clearest blood of a sheepe & put it into a bladder & w^th^ a needle prick holes in y^e^ bottom of it y^n^ hang it up to dry in y^e^ sunne, & disolve it in allum water according as you haue need.

Colours for garmens.

A purple colour.

Take log wood, & seeth it in vinegar & smal beare in an earthen pot, & put a little allum therein till it tast strong.

A red colour

Boyle brasill as you did y^e^ log-wood. but if you would have it a sad red mingle it w^th^ pot ash water, if a light red temper it w^th^ white lead.

A Crimson

Cynaper tops : Cynaper-lake : or vermilion.

A green

Take privet berry water, & chang it w^th^ yellow berry water, & it giveth a perfit greene for y^e^ ground.

Another greene.

Take spannish greene cleane picht [?] & steeped in Rhenish wine, straine it & put unto it a little honey or white suger candey & it will make a good greene.

A light greene.

Temper Verdigrease, & white lead 2 Verdigrease as much yellow berrys & a little white

Yellow colours

Orpiment [?] & saphron, masticol, Gamburgium either of these giv a good yellow.

Blew.

Verditer, Azure or Bice, blew Inde.

Colours for building.

Lay black & white leads for y^e^ walls of the shed, conduits, & greater buildings Bolus for y^e^ pillars & less houses red lead for tiles, for y^e^ leads blew & white, for cottages sut alone.

Colours for Landskip

Lay verditer, blew, white, & greene, or first goo all over it w^th^ saffron & white y^n^ put a little sut to it & goo over it againe. Or first take greene & white leads & goo over it shaddow it w^th^ a little more greene, y^n^ w^th^ white & last of all w^th^ greene, a little white & yellow berrys.

Colours for y^e^ sone beames.

Lay yellow berrys w^th^ a little white shaddow it w^th^ saffron & red lead.

A lyon tawney.

This is made of red lead & Masticot.

A peach colour.

This is of ceruse & Vermilion.

A bras colour.

This is made of Masticot, & umber.

A marble or ash colour.

This is black & white.

A russit colour.

A little white & a good quantety of red.

A browne blew

Take 2 two parts of Inde baudias, & a third of Ceruse

A Crane [?] colour.

It is of black lead & ground w^th^ gum water.

To write gold w^th^ y^e^ pen or pensill.

Take a shell of gold, & put a little gum water into it, & work w^th^ it.

How to write a gold colour.

Take a new laid egg, make a hole at one end & let out y^e^ substance y^n^ take y^e^ yolk without y^e^ white, & four times soe much quicksilver in quantitie as of y^e^ former grind y^m^ well together & put y^m^ into y^e^ shell stop y^e^ hole thereof w^th^ chalk & y^e^ white of an egg y^n^ lay it under a hen y^t^ sitt w^th^ 6 more for y^e^ space of 3 wekes, y^n^ break it up & rite w^th^ it.

ffiesh colour.

Take white lead grind it w^th^ oyle, lake, & vermilion so you may make it pale or high coloured at yo^r^ pleasur.

A white colour.

White lead ground w^th^ nut oyle.

Charecole black & seacole black

Grind charcole very small w^th^ water. Let it dry y^n^ grind it w^th^ oyle. Thus make seacole black.

A colour for silver.

Take charecole blacke & white leade.

A colour for gold

Take Lake vmber red lead & Masticot.

To lay gold on any thing.

Take red lead ground very fine temper it w^th^ Linseed oyle, write w^th^ it & lay leafe gold on it, let it dry, & polish it.

[The running headlines of the pages now change to " Extravagants."]

To melt mettle quickly yea in a shel

Make a bed or laying of metle, & on it make a other bed w^th^ powder of brimstone salt peeter & sawdust. & like quantitie of either then put a fire to y^e^ said powder w^th^ a burning charcole & it will bee in a mass.

To make a hard glow [glew ?].

Take of y^e^ powder of tile sheard two pound unslakt lime 4 pound linseed oyle a sufficient quantaty to temper y^e^ whole mixture. & it is very strong.

To make wood or bone red always

Take y^e^ powder of brasill mingle it w^th^ milk well, but so y^t^ it may be very red. & let y^e^ wood or bone lie 8 days in it.

A good cement for broken glasses.

Take raw silke & beat it w^th^ glasse & mix y^m^ together w^th^ y^e^ whites of eggs.

A bait to catch fish.

Take Cocculus Indicæ ℥ss. henbane seeds, & wheaten flower of each a quart^{r} of an ounce. hive honey as much as will make them into past. Where you see y^{e} most fish cast in bits like barly cornes, & they will swim on y^{e} top of y^{e} water, so y^{t} you may take them up wth yo^{r} hands or a nett. If it rane after y^{e} bate is cast into y^{e} water or if you put y^{m} in othe fare water they will come to themselves.

You may in y^{e} ded of winter in y^{e} morning when y^{e} sun shines catch fish wth yo^{r} angle bated only wth past mad of wheat flower.

To make birds drunk.

Take such meat as they love as wheat, barley, &c. steepe it in lees of wine or in y^{e} juice of hemlock, & sprincle it wher birds use to haunt. Sodden Garlick sprinkled amongst cornes sown.

To cath crows, or ravens.

Take y^{e} liver of a beast & cut it into divers peeces, put some nux vomica into each peece. & lay y^{m} where crows haunt.

To catch crows or pigeons.

Tak whit pease & steepe y^{m} 8 or 9 days in y^{e} gall of an ox. & lay y^{m} where they haunt

To make pigeons, partriges ducks & other birds drunk.

Set black wine for y^{m} to drink where they come.

Another

Take tormentill [?], byle it in good wine put barley into it. Lay it where y^{e} birds com. this should be don in winter when snow is.

Another way to catch birds.

Make past of barley meale onion blade & hen-bane seeds, & set it on severall little boards or tiles, or such like for y^{e} birds.

To make pearles of Chalk

Take some chalke & put it into y^{e} fire there let it lye till it breaks, temper it wth y^{e} whits of eggs & make divers fassions of pearles both great & smal & wet y^{m} being dryed & cover y^{m} wth leaf gold & they are done.

A secret for travellers

Let travelers take a pece of Rock allum, & hold it now & y^{n} for a small time in their mouths for when they are hot it will coole y^{m} & refresh y^{m} & quench their thirst more y^{n} beer will.

There is also a stone (w^{ch} y^{e} Mountebancks call a Celestiall or miraculus stone & y^{e} Apothecarys lapis prunella) w^{ch} dos not much differ from this it is onely better.

A Salve for all sores.

Take a pound of sheeps tallow a pound of turpentine & a pound of Virgin wax, a pint of sallet oyle, a quarter of a pound of Rosin : take also of Bugle, Smallach [in his herb list, smalledge], & plantains halfe y^{e} quantity of y^{e} other or so much as will make a pinte boyle all these together on a soft fire of coles, always stir it till a 3^{d} part be consumed, y^{n} tak it from y^{e} fire & straine it through a new canvas cloth, into an earthen pot.

To make red printing ink.

Take a spoonefull of Vermilion y^{e} quantity of a hazell nut of cleane turpentine wth a spoonefull & a half of linseed oyle grind y^{m} together on a painters stone

To make blew printing ink.

Take bite or smalt & grind y^m^ w^th^ oyle & turpentine as you did for y^e^ former.

To mak yellow printing ink.

Take refin'd Orpiment, & use it as you did y^e^ former.

To make greene printing ink

Take Verdigrease or spanish greene very cleane w^th^ out stalks & grind it as y^e^ former.

To make black printing ink.

Ther is a black earth w^ch^ those y^t^ print maps use & grind it w^th^ oyle & turpentine & you did y^e^ former.

How to write on black paper.

Take y^e^ yolk of a new layd egg & grind it on a marble w^th^ faire water so as you may write w^th^ it. y^n^ write what you will w^th^ it & when it is dry black all y^e^ paper over w^th^ ink & when it is dry, you may scrape all y^e^ letters you wrot of w^th^ a knife.

To ingrave on a flint.

Take a flint & write on it what you will w^th^ y^e^ tallow of an ox, afterward lay y^e^ flint in Vinegar 4 days.

To make the Balsom, of Vincent Lancelles [Lawelles ?], good for all greene wounds, Palsies, Paines proceeding from a cold cause, gouts, straines, Bruises, Aches, Sciatica Cramps. &c

Take of hysop, Margerom [in his herb list, majoram] Tyme, Rosemary, sage, Baume, Wormwood, Rue, Winter, Savoury, Calamint, Southern-Wood, St. John's wort, Penyroyall, Nox, Clowns, All-heale, Sanitle, Selfe-heale, Speedwell, Mother of time, Adders-tongue, ffennel, o each a good handfull, Let them bee all greene, & clense them very well from dirt, & dampe them well in a stone morter, then take a Gallon of the best Sallad Oyle, & put the herbes so bruised, & the oyle altogether in an earthen pot; & add thereunto 3 pints of the best spannish wine, & stir them well together, & cover thē close & then let them stand so in infusion 24 howers; then put them all into a large brass pan, & over a soft gentle charecole fire boyle them till all the humidity bee evaporated, then pres them off very hard into another fit vessell & ȳ take Myrrh 3 ounces, alloes the best & ffrankincense of each 2 ounces, Tathamahara, & Gum-elemy each one ounce, dissolve the Gumms in Sack, & straine them through a linnen cloth. Then take Wax, rosin, Burgundy-pith, & Colophony, of each a pound. Turpentine 4 ounces dissolve them over the fire & mix a little of yo^r^ Oyle w^th^ them wherein yo^r^ hearbs were infused; & then by degrees more & more; & at last all. Then doo soo like wise to gums. ffirst, put in a spoonfull of the Oyle, Wax, & Colophony, mingled together in gums, & stir them well together. then more & more till at last you have incorporated y^m^ all together; then take liquid-slowax [?] 4 ounces, Oyle of spike 3 ounces, Chimicall Oyle of cloves, Nutmegs, Amber, of each two drams; stir them well together, but bee sure you put not in your Chimicall oyles till yo^r^ Barsom bee almost cold.

To make his oyntment for all burning & skalding by fire or Gunpowder. Likewise it aswageth all manner of inflammations, or Gallings by riding or of young Children &c.

Take henbane, hemlock, Lettice, Mandrake leaves, of each one handfull and a half, Poplar buds 4 ounces, houseleeke & Peny-wort of each one good handfull Colerwort [in his herb list, coleworts] leaves 3 good handfull; 7 great old onyons stampe them all in a morter, & put them into two quarts of oyle of violets, & let them infuse 12 howers in warm ashes; then give them 5 or

6 boyles over the fire, then put in halfe a pint of wine-Vinegar, & then byle them again till all the Vinegar bee consumed, then straine them hard off, & ad as much wax to the oyle thus strained off as will give the consistance of an oyntment; then put it up when it is cold, & let it be for yo^r^ use.

A Speciall remedy of his for the tooth-ach, w^ch^ never failed to give ease to any hollow tooth, or other, for a time.

Take Pellitory of Spaine, long peper, Ginger, & Cloves, of each one dram; Ginny-pepper halfe a dram beate all these into a very fine powder; then w^th^ Chimicall oyle of cloves, oyle of thyme, & spirit of salt of each a like quantity, make it up into a past & out of that past make little pellets, or cakes, & dry them in the sun; & so use them.

To make his powder to purge the head.

Take vinger of the best, Orris powder of each halfe a dram; Pellitory of Spaine, & while hellebore, of each halfe a dram; All these into a fine powder & searce [?] them well & add to them two dropps of oyle of Anniseeds. And when you will use it take the quantitie of a barly corne & snuff it upp into yo^r^ nose & it will cause a snezing, whereby it purgeth the head from all superfluous humo^s^ strengtheneth the memory causeth a cleare sight & is good for the thikness of hearing taken as abovesaid every other morning.

An Excelent Plaister of his for cornes.

Take Gum Opoponax, Galbanum, & Gum Elemmy, of each one ounce, of Venus Turpentine 4 ounces, oyle of Mastick 2 ounces, wax two ounces: disolve all these in a bras pan together, & first remember you straine yo^r^ gumms, & then, according to art, incorporate, as you were taught in the Composition of yo^r^ balsom then dip fine clothes in it, & let them ly till they be cold, then when any corne trobles you applie a peice. to it

[At this point in the book the writing shows evidence of some lapse of time. The writing appears to have been more rapidly executed and to give evidence of more maturity. The subject also changes from prescriptions which Newton seems to have received from some physician or chemist, first to helps for the eyesight and then to tricks.]

Helpes for y^e^ eye-sight

Things hurtfull for y^e^ eyes.

Garlick Onions & Leeks: over much Lettice. Gooing too suddaine after meals. Hot wines. Cold ayre. Dunknes. Gluttony, Milke. Chese. White & red coulors. Much sleepe after Meate. Much bloodletting. Cold worts [?]. dust. ffire. much weeping. & watching.

Things good for y^e^ sight.

Measurable sleepe. red roses. ffenell, Selandine Vervaine rootes Pimpernell. Oculus Christi. To wash yo^r^ eyes in faire running water, & yo^r^ hands & feet often. To looke on any greene or pleasant colours, or in a faire glasse.

A Water to cleare y^e^ sight

Take greene Wallnuts husks & all as they come from y^e^ trees & a few of y^e^ leaves & distill them. Then drop y^e^ water thereof into yo^r^ eyes morning & night for 6 or 7 days together.

Drop 3 or 4 drops of y^e^ water of rotten apples at a time into yo^r^ eyes.

Wash yor eyes wth ye water of Dasies, both roots & leaves being cleane washed, then stamped & so strained.

The juice of ye hearbe Euphasia, or ye water.

Another

Take a new laid egge, roast it harde cut ye shell in ye midst & take out ye yolke cleane & put a peice of copporas in wher ye yolke lay, binding ye egg together again yn set it to ye fire till ye copporas bee disolved to water. yn take all ye white out of ye shell cutting it in small peices, yn put it into a glas of faire running water letting it stand so a little while. yn straine it through a fine lining cloth, & keepe it in a glass close stopped so wash yor eyes every morning & evening therewith.

Certaine tricks

To turne waters into wine

1 Into Claret

Take as much bockwood as you can hold in yor mouth wth out discovery tye it up in a cloth, & put it in yor mouth, then sup up some wather & champe ye bockwood 3 or 4 times & doe it out into a glass.

2 ffor White wine.

Chew ye Ball once or twice lightly, &c as you did for claret.

ffor Sack.

Take a drop of Wine or beare vinegar & put it in ye Glasse shakeing it about ye sides of ye Glasse, &c: as you did for claret.

ffor Sky coloured Blew or Put a corne of salt in ye Glasse &c: as you did for claret.

ffor posset drinke & curds.

Take one drop of Sallet oyle, &c as you did for sack. & it will be posset drinke, yn let it stand a while & it wil be curds.

ffor Strong Waters.

Have a cup of Strong Water by you like ye other of water wch drinke up as if it was ye water & doo it out againe into one of ye Glasses.

To Cut a Glasse.

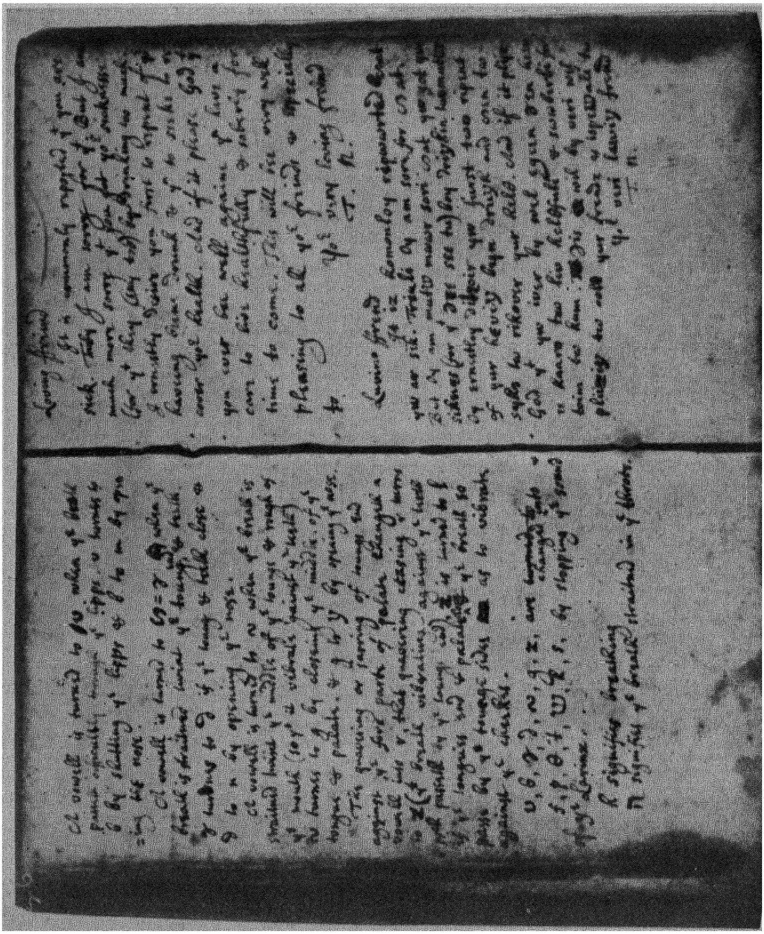
Take a plaine Glasse, hold it up side downeward over a candle till it bee pritty hott then take match of rope, & blowing it all ye while run it ovr ye glasse as you would have it cut.

A Remedy for a Ague

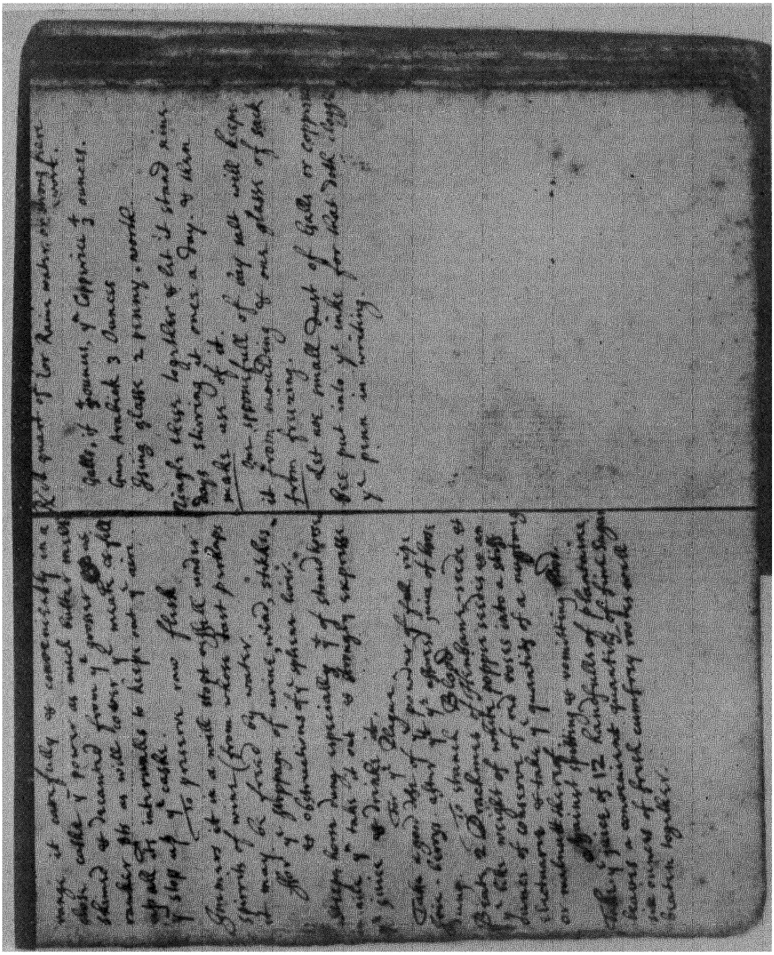
When Jesus saw ye Cross.

[Then follow six lines of symbols, presumably the secret writing of some incantation.]

The rest of the notebook can best be understood by a few facsimiles. The first one shows Newton's interest in spelling reform, the Hebrew forms showing some slight familiarity with that language. It will be sufficient to give only two of the six pages which he devotes to this subject.



Some idea of that part of the notebook which seems to have been written about the time he went to Cambridge may be derived from the following pages of recipes:



mage it carefully & conveniently in a
close caske & powre as much butter melted
skimed & decanted from ye grosser [illegible]
rancker [illegible] as will cover ye meate & fill
up all its intervalls to keepe out ye aire
& stop up ye caske.

To preserve raw flesh

Immerse it in a well stopt vessell under
spirits of wine (from whose tast perhaps
it may be freed by water.

For ye stoppage of urine, wind, stitches,
& obstructions of ye spleene liver.

Steepe horse dung especially yt of stoned horse
in ale yn take it out & strongly expresse
ye juice & drinke it.

For ye Plague.

Take a good dose of ye powder of full ripe
[illegible] berrys after yt ye aforesaid juice of horse
dung.

To stanch Blood.

Beate 2 Drachmes of Henbane seede &
ye like weight of white poppie seedes [illegible] an
ounce of conserve of red roses into a stiff
electuary & take ye quantity of a nutmeg
or walnutt thereof.

Against spitting or vomiting blood.

Take ye juice of 12 handfulls of plantaine
leaves a convenient quantity of fine Sugar
six ounces of fresh cumfrey rootes well
beaten together.

[illegible] quart of [illegible] Raine water, or strong faire
wort.

Galls, 1½ ounces, yn Copperice ½ ounces.

Gum Arabick 3 Ounces

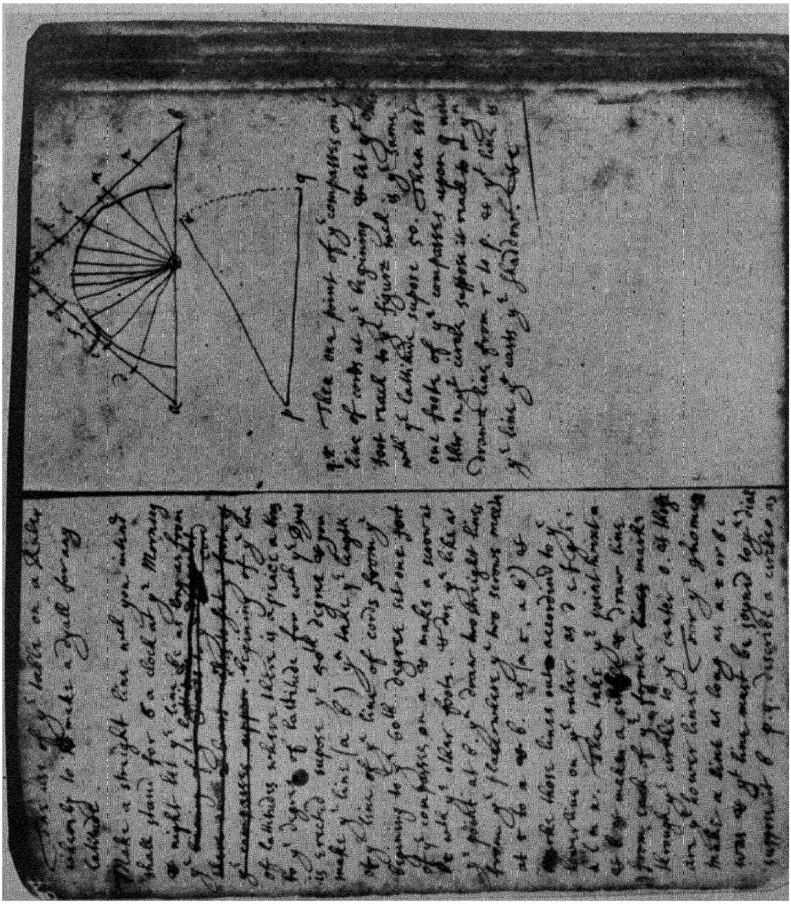
Ising glasse 2 penny. worth.

Mingle these together & let it stand nine
Days stirring it once a Day. & then
make use of it.

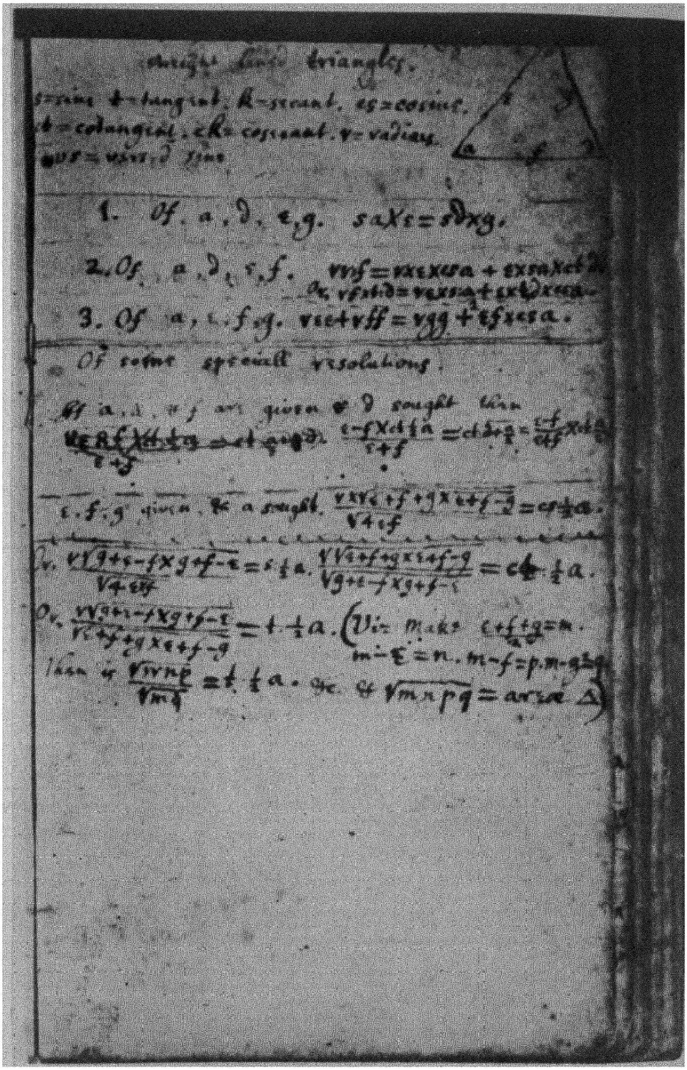
One spoonefull of dry salt will keepe
it from moulding & one glasse of sack
from freezing.

Let noe small Dust of Galls or Copperis
bee put into ye inke for that doth clogge
ye penn in writing.

There are two pages on dialling, as here shown :



The pure mathematical part consists of five pages of which one is here given.



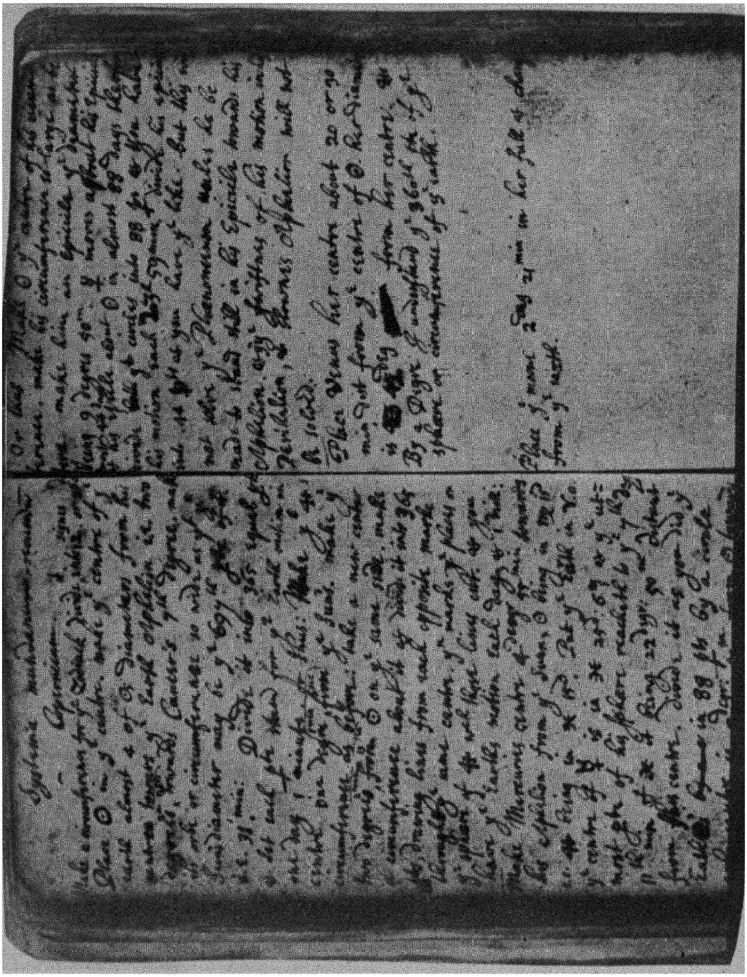
s = sine. t = tangent. k = secant. cs = cosine. ct = cotangent. ck = cosecant. r = radius. vs = versed sine

1. Of a, d, e, g. $s\,a \times e = s\,d \times g$.

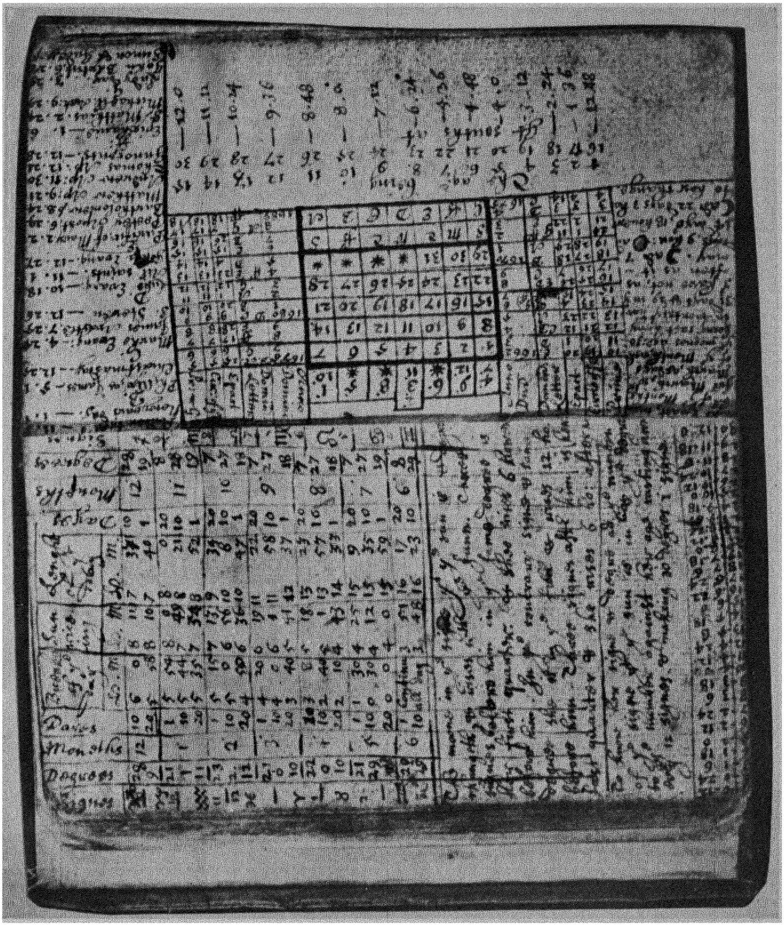
3. Of a, e, f, g. $ree + rff = rgg + 2ef \times csa$.

Of some speciall resolutions.

He has two pages of notes on the Copernican System, as follows, the handwriting being that of the middle period referred to above:



His work on the ecclesiastical calendar begins with the year 1662, which was probably a year or so in advance. Two of the six pages of this work are as follows :



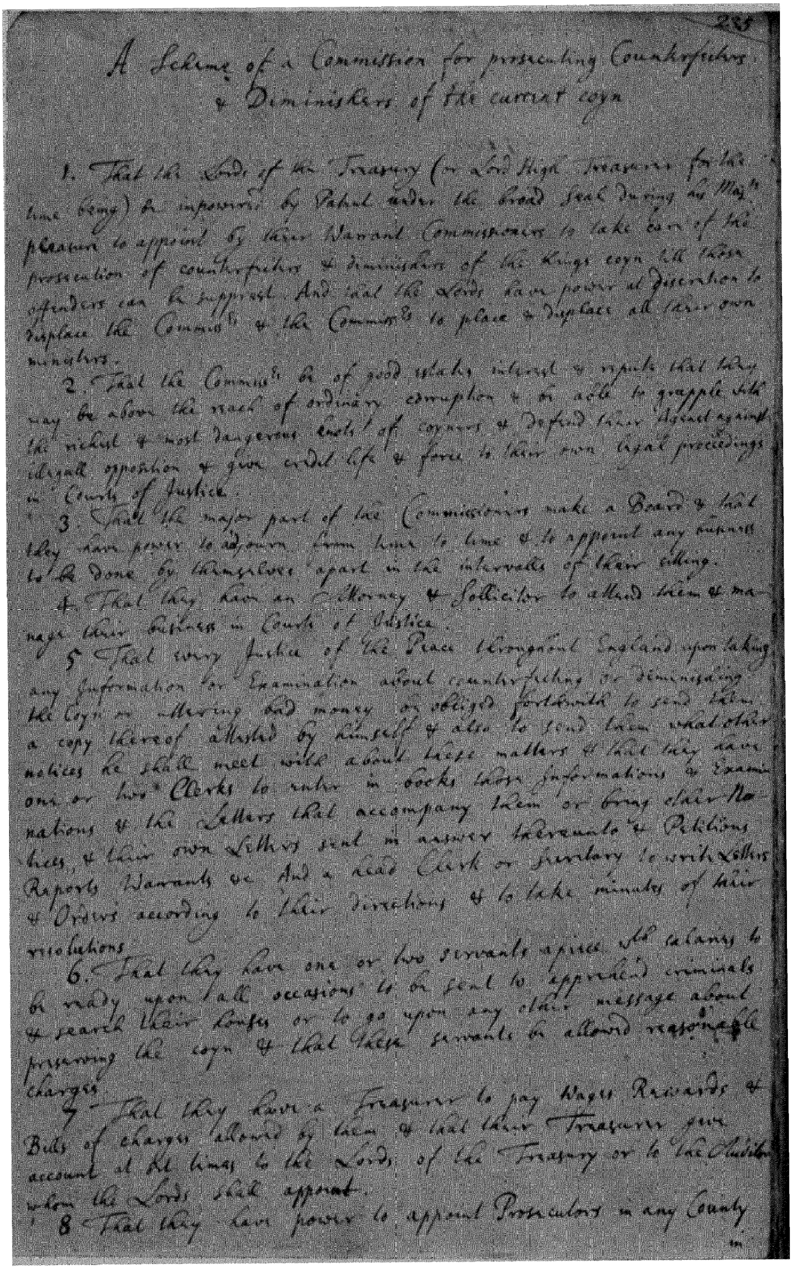
As is the case with at least two other notebooks of Newton's, this begins at both ends and proceeds towards the middle.

There is nothing in the Pierpont Morgan library to indicate the owners of the book from the time when Mr. Conduit examined the papers to the date of its purchase by Mr. Morgan.

The second document referred to at the beginning of this article is as follows :

A Scheme of a Commission for prosecuting Counterfeiters & Diminishers of the current coyn.

1. That the Lords of the Treasury (or Lord High Treasurer for the time being) be impowered by Patent under the broad seal during his Maj^{ts} pleasure



235

A Scheme of a Commission for prosecuting Counterfeiters & Diminishers of the current coyn

1. That the Lords of the Treasury (or Lord High Treasurer for the time being) be impowered by Patent under the broad Seal during his Maj^ts pleasure to appoint by their Warrant Commissioners to take care of the prosecution of counterfeiters & diminishers of the Kings coyn till those offenders can be suppressed. And that the Lords have power at discretion to displace the Commiss^rs & the Commiss^rs to place & displace all their own ministers.

2. That the Commiss^rs be of good estates interest & repute that they may be above the reach of ordinary corruption & be able to grapple with the richest & most dangerous knots of coyners & defend their Agents against illegall opposition & give credit life & force to their own legal proceedings in Courts of Justice.

3. That the major part of the Commissioners make a Board & that they have power to adjourn from time to time & to appoint any business to be done by themselves apart in the intervalls of their sitting.

4. That they have an Attorney & Sollicitor to attend them & manage their business in Courts of Justice.

5 That every Justice of the Peace throughout England upon taking any Information or Examination about counterfeiting or diminishing the Coyn or uttering bad money be obliged forthwith to send them a copy thereof attested by himself & also to send them what other notices he shall meet with about these matters & that they have one or two* Clerks to enter in books those Informations & Examinations & the Letters that accompany them or bring their Notices, & their own Letters sent in answer thereunto & Petitions Reports Warrants &c And a head Clerk or Secretary to write Letters & Orders according to their directions & to take minutes of their resolutions.

6. That they have one or two servants apiece with salaries to be ready upon all occasions to be sent to apprehend criminals & search their houses or to go upon any other message about preserving the coyn & that these servants be allowed reasonable charges.

7 That they have a Treasurer to pay Wages Rewards & Bills of charges allowed by them & that their Treasurer give account at all times to the Lords of the Treasury or to the Auditor whom the Lords shall appoint.

8 That they have power to appoint Prosecutors in any County

in

to appoint by their Warrant Commissioners to take care of the prosecution of counterfeiters & diminishers of the Kings coyn till those offenders can be supprest. And that the Lords have power at discretion to displace the Commiss^{rs} & the Commiss^{rs} to place & displace all their own ministers.

2. That the Commiss^{rs} be of good estates interest & repute that they may be above the reach of ordinary corruption & be able to grapple with the richest & most dangerous knots of coyners & defend their Agent against illegall opposition & give credit life & force to their own legal proceedings in Courts of Justice.

3. That the major part of the Commissioners make a Board & that they have power to adjourn from time to time & to appoint any business to be done by themselves apart in the intervalls of their sitting.

4. That they have an Attorney & Sollicitor to attend them & manage their business in Courts of Justice.

5 That every Justice of the Peace throughout England upon taking any Information or Examination about counterfeiting or diminishing the Coyn or uttering bad money be obliged forthwith to send them a copy thereof attested by himself & also to send them what other notices he shall meet with about those matters & that they have one or two Clerks to enter in books those Informations & Examinations & the Letters that accompany them or bring other Notices, & their own Letters sent in answer thereunto & Petitions Reports Warrants &c. And a head Clerk or Secretary to write Letters & Orders according to their directions & to take minutes of their resolutions.

6. That they have one or two servants apiece wth salaries to be ready upon all occasions to be sent to apprehend criminals & search their houses or to go upon any other message about preserving the coyn & that these servants be allowed reasonable charges.

7 That they have a Treasurer to pay Wages Rewards & Bills of charges allowed by them & that their Treasurer give account at set times to the Lords of the Treasury or to the Auditor whom the Lords shall appoint.

8 That they have power to appoint Prosecutors in any County in England & to allow them reasonable charges & such further encouragements as they shall find sufficient.

9. That they have power to appoint who shall be prosecuted & who shall be witnesses & as much power as the King can give them to defend the Witnesses from being prosecuted for these matters unless by order of the Attorney Gen, & to terrify criminalls & their friends [*from being prosecuted f* ,—these italics are cancelled in the document] & principally their Sollicitors from vexing threatening or tampering with the witnesses.

10 That suitable rewards be proposed either by his Maj^{ts} Proclamation or by the Order & at y^{e} discretion of the Commissioners to them that shall discover coyning shops & apprehend the owners or other coyners imployed therein, or the Engravers of their stamps.

11. That the Commissioners Prosecutors & Witnesses be not suffered to meddle with the forfeited estates of these criminalls excepting their coyning tools & counterfeit & light money & such other things as are by way of evidence to be seized by the warrants of Justices of the Peace & produced in courts of Justice.

12. That all milled money (gold or silver) w^{ch} wants any part of the Letters or any part of the space where the letters should stand or the greater part of the indented ring about the letters on either side the coyn be called in by his Maj^{ts} Proclamation & that y^{e} Commiss^{rs} be obliged to receive the same or any other money of standard gold or silver otherwise diminished then by wearing, at the rate of ꝑ ounce of standard silver & of

ꝑ ounce of standard gold & also to receive all counterfeit money at y^{e} rate of ꝑ ounce of standard silver & of ꝑ ounce of standard gold, & to recoyn at their own charge what ever they shall so receive & that for this undertaking the Commissioners be allowed by the King after the rate of ꝑ annum over & above their salaries & his Maj^{ty} be gratiously pleased not to pardon any offenders without the advice of the Commiss^{rs}. For this Constitution will make it the interest of the Commiss^{rs} to serve the King with fidelity & vigour.

13. That upon complaint made against any Justice of the Peace the Commiss^{rs} be impowered to appoint by Warrant under their hands & seals any other two or more Justices of the Peace severally to summon & jointly to examin the parties & their Witnesses upon a day prefixt in the County & Town where the complaint doth lye or in the next mercal town & to report the matter to the Commiss^{rs} & they to the Lord Chancellour if the Justice complained of be found faulty. For this will quicken Justices of Peace & give the Commiss^{rs} authority.

The document is endorsed in another but contemporary hand: "Scheme of a Com̃ission for prosecuting Clippers and Coyners."

LETTERS FROM NEWTON
IN
CORPUS CHRISTI COLLEGE, OXFORD.*

Communicated by the late J. L. E. **Dreyer**, **Ph.D.**, D.Sc.

For Mr. Crompton to be sent to Mr. Flamstead.

I thank Mr. Flamstead for his kind mention of me in his letters to Mr. Crompton. And as I commend his wisdom in deferring to publish his hypothetical notions till they have been well considered both by his friends & himself, so I shall act the part of a friend in this paper not in objecting against it by way of opposition but in describing what I imagine might be objected by others & so leaving it to his consideration. If hereafter he shall please to publish his theory & think any of the objections I propound need an answer to prevent their being objected by others, he may describe the objections as raised by himself or his friends in general without taking any notice of me.

First then whereas in the annexed figure he supposes that when the earth is in ♊, the comet moved from *B* to *f* & so to *D* & then to *E*, so as to be in its perige twice, the first time at α when the earth was in ♊, the next time at β when the earth was in ♋ & in its perihelion at *D* to be between the ☉ &

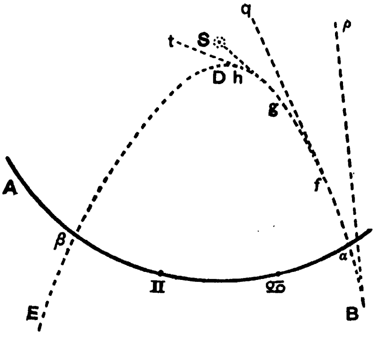


FIG. 1.

---

* Four letters written by Isaac Newton to Flamsteed. Copied from the originals belonging to Corpus Christi College, Oxford. The spelling has not been changed, except that I have written *the* for y^e (a few times N. has written " the ") and F instead of ff. Otherwise it is an exact copy.

Pages 1-10, 28 Feb. 1680. Partly printed in the *Biog. Brit.*, *i.e.* from near end of p. 3221, foot of col. 1, " I am further suspicious . . . "

Pages 11, 16, 16 Apr. 1681. *B.B.* v. p. 3223 *sqq.*

Pages 17, 18, 1685 or Jan. 86. *B.B.* v. p. 3228.

Page 19, 30 Dec. 1686. *B.B.* v. p. 3228.

The first two are alluded to by Brewster, who gives short quotations, i. p. 301 *sq.*

earth: draw *Bp*, *fq*, *gs*, *ht* tangents to the line of the Comets motion at *B*, *f*, *g* & *h*, & I can easily allow that the attractive power of the sun as the comet approaches the sun passing from *B* to *f* & then to *g*, will make the comet verge more and more from its former line of direction towards the sun, so that its line of direction which at *B* was *Bp*, at *f* shall become *fq*, but I do not understand how it can make the Comet ever move directly towards the ☉ as at *g* where the line of direction *gs* passes through the center of the Sun, much less can it make the line of direction verge to the other side of the sun as at *h* where the line of direction is *ht*. For if the Comet at *g* moved directly towards the ☉ & the ☉ also attracted it directly towards himself it would continue to go towards the ☉ in the line *gs* till it fell upon the ☉, there being no cause to turn it out of the line of its direction *gs* towards *h*. The case is as if a bullet were shot from west to east. The attraction of the earth by its gravity will make the bullet tend more & more downwards, but it can never make it tend directly downwards much less verge from east to west. Nor will the motion of the vortex releive the difficulty but rather increase it. For that being according to the order of the letters & marks *Aβ*♊♋*α* would make the comet verge from the line *gs* rather towards the line *fg* than towards *h*. The only way to releive the difficulty in my judgm^{t} is to suppose the Comet to have gone not between the ☉ & earth but to have fetched a compass about the sun as in this figure.

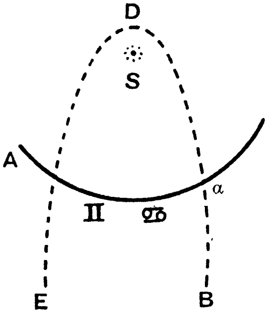


FIG. 2.

Secondly though I can easily allow an attractive power in the ☉ whereby the Planets are kept in their courses about him from going away in tangent lines, yet I am the lesse inclined to beleive this attraction to be of a magnetick nature because the ☉ is a vehemently hot body & magnetick bodies when made red hot lose their vertue. A red hot loadstone attracts not iron nor any loadstone a red hot iron, nor will a loadstone propagate its vertue through a rod of iron made red hot in the middle. Whence probably the Earth were it made red hot would lose its magnetism; & the sun being more than red hot, must be less capable of it.

But thirdly were the ☉ a magnet he would act on the Comet as a great magnet does on a little one floating in a wallnut shell or other little boat on water. If the little magnet were forcibly turned about by one's hand and put into a wrong or unagreable position the great magnet so long as the little one was forcibly detained in that wrong position would repell it, but so soon as the little magnet was set at liberty the great magnet would make it nimbly turn about into an agreeable position and then attract it. And so the sun, were the comet in a wrong position, would make it turn about quickly into a right one & then attract it & keep it constantly in that right position. How then the Comet being at first in a right position so as to be attracted by the Sun should afterwards get into a wrong one so as to be repelled from him

I do not conceive. For the directive vertue of a great magnet is stronger than its attractive vertue. The mariners needle is not sensibly attracted by the earth, but it's strongly directed by it, so that you cannot make it stand in a wrong position. If then the Sun be a magnet, the axis of the Comet ought to be strongly directed by him into such a position as the laws of magnetism require, & being so directed the Comet will be always attracted by the sun & never repelled. And so if the axes of the Planets be inclined to the ecliptick by the Sun's magnetism, they ought to be so directed as the laws of magnetism require, which I fear they are not.*

I am further suspicious that the Comets of November & December which Mr. Flamstead accounts one & the same comet were two different ones, & I find Cassini in a copy of a letter of his which Mr. Ellis shewed me is of my mind. If they were but one Comet, its motion was thrice accelerated & retarded, from Nov. 18 to Nov. 21 it moved after the rate of almost six degrees a day. From Nov. 23 to Decemb. 5 after the rate of but 36 minutes a day. From Decem. 6 to Decemb. 12 after the rate of almost 8 degrees a day.† From Decemb. 12 to Decem. 19 after the rate of about 3½ degrees a day. From Decem. 24 to Decem. 26 after the rate of almost 4½ degrees a day. From which time the motion decreased continually. This frequent increas & decreas of motion is too paradoxical to be admitted in one & the same Comet without some proof that there was but one. Besides it is very irregular. For after the 20th day of November when the Comet was in its first perige ‡ as Mr. Flamstead notes, & moved after the rate of about six degrees a day, that its motion should suddenly decrease so much as that from Novemb. 23 to Decemb. 5 to move but 7¼ degrees & consequently in the middle part of that time (suppose at Novemb. 29 or 30) to move after the rate of less than half a degree a day, & this while the Comet is going towards the Sun & so has its real motion continually accelerated, is very odd, & makes me question Father Gallet's observations on which the supposition of but one Comet leans. And then that the motion should as suddenly increase so as from Dec. 6 to Dec. 12 that is in six days to move a signe & a half & consequently in the middle of that time (suppose at Dec. 9) to move after the rate of 9 or 10 degrees a day at least, & this when the comet was gone much further from us, is more odd than the former, not to mention the extravagant deviation from a great circle & cutting the ecliptick twice.

There is one thing more which increases this difficulty. Mr. Flamstead Decemb. 10 observed the tail of the Comet to point between the sholder & tayle of Aquila. Draw a line from thence to the sun & produce backwards the way which the Comet described afterwards till it cut this line & the intersection point will very nearly be the place of the comet Decemb. 10. This point is scarce six degrees from the place of it Decemb. 12, but above 40 degrees from the place of the comet observed by father Gallet Decemb. 6 hora 18 that is Decemb. 7 at 6 a clock ith' morning so that if these comets were the same, it moved but 7¼ degrees from Nov 23 to Dec 5 that is in 12 days & then in its recess from the earth advanced its motion on a suddein so as to move 40 degrees from Decem 7 ith morning to Decem 10 at night that is in three days & a half, & again on a suddein in coming towards the earth abated its motion so as to move but 6 degrees or less from Decemb 10 to Decem 12.

The good agreem^{t} between Mr. Flamstead's & the Parisian observations prove 'em both sufficiently accurate, but I know not what to think of Father Gallet's. A schollar of o^{r} College with some others saw the Comet in November. About the middle of December one of o^{r} Fellows sent for him &

---

* From here printed in *B.B.* vol. v., but with accidental variations.

† "From Dec. 6th to Dec. 19th, after the rate of about 3½ degrees a day." *B.B.* p. 3221, fn. col. 2.

‡ position. *B.B. loc. cit.*

examined him about it & he described to him that it passed by Spica Virginis towards the Sun. About a month since I sent for him & upon a strickt examination he told me that on Tuesday Novemb 16 he saw the Comet at four a clock in the morning two degrees above Spíca with a long tail extending to the Lyons tayle. The tayle of a silver white colour, more white than the tayle of the Comet in December. I asked him if it were directly over spica he said it declined a little toward one side & but a little & to the best of his remembrance it declined eastward. I asked him if it was not above two degrees from Spica, & if he were sure the star by it was spica : he said the starr was such a noted one y^t^ he could not be mistaken in it & for the distance it was hard by the starr so that he recconned it but about two degrees from it, though he would not be positive but that it might be four or five degrees from it, a degree or two being easily mistaken in judging by the eye, but it was hard by Spica & its tayle was long & reached to the bright star in the Lyons tayle. He said further that four days after, on satturday Novem 20 he saw it again a little before sun rise, advanced a good way from Spica towards the Sun. Its tayle was not now visible except a few rays w^ch^ went almost towards Spica nor did any Starrs but Spica appear, so that he could not say what fixt starrs it was neare nor what its place was, only he thought it was not far from the Ecliptick. Taking a little pespective out of his pocket he viewed it & saw it not like another starr but like a little round cloud. I was the more scrupulous in examining this scholar because I knew not what to make of these things they not agreeing to the Comet of December. And when he saw me at a puzzel he was concerned & added that there were divers other scholars who saw it with him. Now how to reconcele the former part of this relation with father Gallet I know not unless either he or the scholar have mistaken a day or two. And yet they will not agree. For by the position of the tayl in the scholars first observation one may conclude that the Comet was then on the north side the Ecliptick & crossed not the ecliptick till after the 20th degree of Libra, whereas Father Gallet makes it cross in the 8th degree.

Further one of o^r^ Fellows, Dr. Babington, between the 20th & 27th of November, & to the best of his memory about the 22th sr 23th day at 5 a clock ith' morning or between 5 & 6 saw the tayle of the Comet shoot over Kings College Chappel from east to west. Twas a frosty morning & a very clear & starry sky. The head of the Comet he saw not. The tayl ran from one end of the chappel to the other just above the chappel rising a little higher at the west end than at the east as in this figure. By considering the place

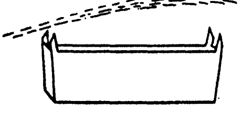


FIG. 3.

where he stood, the chappel there appears about 20 degrees long & stands almost full south declining a little to the west. So that the tayl shot a good way beyond the meridian westward, suppose 12 or 16 degrees at least, & was 20 degrees long besides that part of the tayl w^ch^ ran from the east end of the chappel to the head w^ch^ seems to have been between 20 & 30 degrees more. The chappel there appears about 7 or 8 degrees high, & consequently the tayl shooting just over the chappel as he described it to me could not be above 10 or 12 degrees high where it cut the meridian nor above 14 or 16 or at most 18 degrees high at the west end, while it may be gathered that it shot on through the beak of corvus close under the cup to some degrees beyond the cup, or thereabouts: & consequently that the head (which was either under the Horizon or so neare it that the D^r could not see it for buildings) was advanced at this time considerably more in southern latitude than Father Gallet makes it at any time, & so probably within a few days after this, might go so far southward as to be seen by us no more. At least it may hence be suspected that Gallets last observations make the south latitude something too little. Perhaps if inquiry be hereafter made of those wch come from beyond the line, something further may be learnt of this comet. And if M^r Flamstead examine Cuthbeard or others who saw it in November, he may perhaps gather something from them.

One circumstance there is more for evincing two Comets. That in November had a brighter head than the other, as the schollar aforesaid told me & this circumstance evinces that it was easily seen Novem 20 * when day break was so far advanced that neither the tail nor any starrs but those of the first magnitude could be seen.

Mr. Flamstead ingeniously gives a reason why Comets usually verge from south to north. But I suspect the Phænomenon. Those wch go southward are seldome discovered till they be ready to cross the Eclipticк & then turning their tayl downwards are obscured by the thick air neare the Horizon so as to be little seen afterwards, which makes 'em rarely noted by the vulgar, or recorded by the ancients who set down only the more famous ones. Since Comets have been more strictly looked after, there has as many gone southward as northward. For the nine Comets wch have appeared in the years 1647, 1652, 1661, 1664, 1665, 1666, 1677 † & 1680, five if I mistake not, went southwards, namely those of the years 1647, 1665, 1666, 1677 ‡ & the former of the year 1680. And I think I may add a sixt, one of those two w^ch anno 1652 were seen in the west Indies, whereof but one was seen in o^r parts. So then more Comets have of late gone southward than northward, and yet these appeard so little to us, that had they been in former ages, scarce any but that of 1665 would have been recorded.

Mr. Flamstead gives an ingenious guess at the reason of the Comets atmosphere, by supposing the Comet covered over w^th water & consequently more vapors raised than in o^r Planets. But o^r earth is half covered over with water & so is the Moon in a manner & were they covered all over their atmospheres therefore would not be more than doubled, whereas the Comets atmosphere is some hundreds not to say 1000^s of times greater in proportion to the body.

And the atmosphere being so vast its probable that neare the body of the Comet it is thicker and grosser than ours in the most foggy misty days we have & consequently that the sun never shines distinctly through it, nor we distinguish the more & less lucid parts of the body through it. Fôr if the atmosphere was so thick that the limb of the body could not be seen through it distinctly, much less could the fainter & smaller lineaments of land & sea, w^ch even in our Planets (Iupiter Mars Venus &c) whose atmospheres are fine & thin & limbs appear distinct, is scarce discernable.

This is the summ of what I can think of to object, w^ch I presume M^r Flamstead will not be displeased w^th, it being an advantage for any man to have a prospect of what objections ly against him before he divulge his thoughts that so he may frame & propound them after the best manner.

Besides these things w^ch seem obnoxious to some objections, there are some other parts of his Theory w^ch I easily approve & think will be hard to object against, as y^t the tayle of the Comet is a thin vapour, that it rises from the atmosphere about the head, that the action of the sun's light conduces to raise it, that it shines not by its own light but only by reflexion of the sun's light, and that the atmosphere about the head shines also by the suns light, though perhaps not altogether by it. But I am too tedious.

---

* November, 29th. *B.B. loc. cit.* † 1667. *B.B. loc. cit.* ‡ 1667. *B.B. loc. cit.*

I would gladly know whether on Decemb 12 when Mr Flamstead first espied the head of the Comet, he could see it so near the sun with his naked eyes or only through glasses, & to a starr of what magnitude he thinks it might then be compared. Also whether he allowed for refraction in computing its place at that time. Also what starrs the middle of the tayl passed through & was directed towards upon the 10th, 11th, 12th, 15th & 21st days, for I did not then mind it. I am told the tayl on the 12th day went to the bill of Cygnus on the 15th to the Ancon alæ austrinæ Cygni, but Mr Flamstead can inform me better. I would gladly know what day also he saw the comet with little glimpses of light like[*] sparks & whether he saw it so any more before & after & what variations he observed in the body as to its light & dark parts. Cysatus & Hevelius describe great and perpetual variations but I had rather rely on Mr Flamstead's authority. I would enquire also whether the said lucid points on the body were all[†] over the head or only on that hemisphere towards the sun & whether he could perceive that hemisphere towards the ⊙ any more lucid than the opposite hemisphere, & what he thinks of Hevelius observations of the phænomena of the head. But I fear I shall be troublesome.

On friday last I saw the Comet neare the western of the two starrs in the left foot of Perseus & last night I saw it again. It passes in a line about half a degree to the north side of the two starrs & on satterday was got over against the western of them. In the annexed figure *A* represents the western, *B* the eastern of the two starrs, and the mark ◌ the Comet Feb 25 & 27.[‡] I noted its position to the telescopical starrs but have not yet determined the position of the starrs one among another, & so cannot yet be accurate in describing its place. But by this gross description Mr. Flamstead will easily find it if he have not yet seen it since the last moon. But its now grown so faint that to see it well he must give his glass a large aperture. After another clear night or two he may command the observations I have made since the last full moon together with those I shall further make, to compare with his own if he please. In the beginning of Ianuary the whole coma of the Comet seemed to me to be about 12′ or 15′ broad but I could not observe the diameter of the body or nut in the center of the coma. If Mr Flamstead observed it, the communication would be gratefull. But chiefly I would beg of him a description (in words) of the position of the tayle to the fixt starrs Decemb 10, 11, 12, 15 & 21. For by a thought wch came into my mind to day, I hope there may be something gathered from that. I have been too tedious & shall add no more but my thanks to Mr Flamstead for the kind communication of his Philosophical notions.

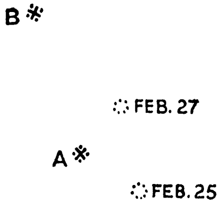


FIG. 4.

Cambridge
Feb 28. 1680
Recd March 4
1680

Is. NEWTON.

Trin. Coll. Apr. 16. 1681

Sr

Tis now almost three weeks since upon my return from a journey I received yors. By some imdisposition & other impediments I have deferred answering you longer than I intended. For I was desirous to return you quicker thanks for your kind communications. The complements you are pleased out of extream kindness to give me, might better have suited wth your self from me; nor do I think it suits wth me to judge of an Hypothesis after your thoughts upon it. I only propounded objections for you yo^{r} self (if you had not thought on 'em before) to judge further of it by w^{ch} therefore being designed only for your use I forbear to urge further & shall only speak to your question of two Comets. The mistake about the date of Pere Gallet's observations was in the copy I received of them. In the title 'twas writ *Stylo veteri*, & accordingly the observations all but the two last were altered from the new style to the old one & put Novemb 17, 18, 21, 24, 25, Decem: 6, 7. And the like alterations from the new to the old style had the French observations under them. I perceive the scholar in his observation of the transit of the Comet by Spica was mistaken in the time. He recollected it only by circumstances & it seems told me Novemb 16 for Novemb. 19. In your argument from the consent of elongations, if you estimate the motion of the Comet not in the Ecliptick but in the line of its proper motion (as I think should be done) you will find some difference between November & December. But that is not material, the apparent celerity depending on the earth's distance from the Comet at those times, & so whether equal or unequal being but accidental. You seem to incline to think the way of the Comet wider then in yo^{r} scheme & so do I for I apprehend the Comet in M^{r} Halleys & yo^{r} first observations Dec 8, 10, 11, 12 to have been remoter from us then the ⊙ & about Ian. 2 to have been as far from the ⊙ as the earth was & its Heliocentrick place then to have been ♊ 9^{degr} with north latitude 19 or 20^{degr} or there abouts. But you are afraid the long tail will not admitt the Comets passing beyond the ⊙. I apprehend nothing from thence, for I am forced to beleive the tail extended beyond the sphere of ♂ during the whole appearance of the Comet & so was long enough to appear in the beginning of December as long as it did. Nor was the apparent length of it at that time any way enormous but consonant to the law it observed all December for the tail all that month & (by my observation some days in Ianuary) ended at a great circle w^{ch} cut the Ecliptick in ♐ 19½^{degr} at an angle of about 52^{degr}. Now if the December Comet was beyond the ⊙ in the beginning of December, the Comets of November & December could hardly be the same for this reason. Let *EDF* represent the line the Comet moved in, *S* the Sun *CD* the axis of that line passing through the Sun, *D* its vertex or Perihelion of the Comet, and *AB* the plane of the Ecliptick crossed by the axis in *S*. By contemplating the figure you will perceive the vertex or Perihelion *D* was on the south side the Ecliptick & consequently since the comet crost the Ecliptick about the 3d or at latest the 4th of December it was in *D* a little before that time viz above half a day before, for the angle *DSB* was about 6 or 8 degrees at least. But in conjunction with the Sun it was Decemb. 9. So then in

---

[*] *like* omitted. *B.B.* p. 3222.

[†] *also* for *all*. *B.B. loc. cit.*

[‡] This sentence omitted. *B.B.* 1223.

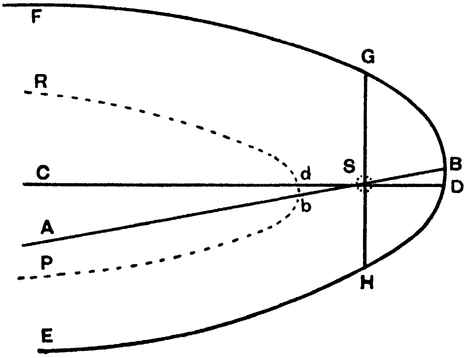


FIG. 5.

passing from D to conjunction there were scarce less then six days spent. But in the Hypothesis the point D was opposite to the earth about Decemb. 7 & consequently Decemb 9 was so neare to opposition that is to conjunction with the ⊙ that the Comet in passing thence to conjunction could not have spent many hours. Again drawing GSH perpendicular to CD, GS being equal to SH, the Comet should have had as much latitude in H as in G. In passing from conjunction to G it must have spent much more time then in passing from D to conjunction & consequently could not be in G before the 12th day if so soon & so in G had 8 or 9 degrees north latitude at least, but in H by the Roman observations as they are adapted to the Hypothesis could not have above a degree south latitude.

If the Comet turned short of the ⊙ suppose in the line PQR, the difficulties are thereby something diminished, but I think not taken of. The point d or vertex of the figure described by the Comet was in conjunction with the ⊙ Decemb. 7. The Comet in conjunction Decemb 9. Therefore the comets conjunction happened on that side d towards b. The Comet was in b (the point where it crost the Ecliptick) Decemb 3 or 4. Therefore it spent 5 or six days in passing from b to a point between b & d: wch space by the Hypothesis is yet so little that the comet could not spend many hours in passing it. And I think too the south latitude though it could not be so great as the north yet it ought to have been greater then the Roman observations Nov 26 & 27 make it.

But what ever there be in these difficulties, this sways most with me that to make the Comets of November & December but one is to make that one paradoxical. Did it go in such a bent line other comets would do the like & yet no such thing was ever observed in them but rather the contrary. The Comets of 1665, 1677 & others which moved towards the Sun, or some of them at least, had they twisted about the ⊙ & not proceeding on forward gone away behind him they would have been seen again coming from him. The many wd have been seen advancing from the ⊙, or some of them at least would have been seen in the former part of their course advancing towards him had that former part been performed, not in the line of the latter part shooting on backwards towards the regions beyond the ⊙ but twisting about him towards any hand. Those which were seen both before & after their perihelium's as the comets of 1472, 1556, 1580 & 1664 would not as they did, have begun in our part of the heavens & ended in the opposite part, going through almost a semicircle wth motion first slow then swift then slow again as if done in a right line, had it been done in such a line as the Hypothesis puts. Let but the Comet of 1664 be considered where the observations were made by accurate men. This was seen long before its Perihelium & long after & all the while moved (by the consent of the best Astronomers) in a line almost straight. So neare was the line to a straight one that Monsr Auzout on supposition that 'twas an arch of a great circle about the dog starr (as Cassini guessed & Auzout was afterward willing should be believed) or rather a straight one (as the obviousness of the Hypothesis, easiness of the calculation & number of observations on wch 'twas founded makes me suspect) did from these observations predict the motion to the end without very considerable error.

But you ask why the Comet of November staid so long in the same southern latitude if it turned not back. I am not satisfied that it did so. I fear twould be hard to warrant any of the observations of that Comet to less then a degree, & why then might it not have in the time of the Canterbury observation between one & two degrees north latitude, & afterwards crossing the Ecliptick about the beginning of the Roman observations, as Gallet makes it, & from thence advancing continually southward, arrive to between one & two degrees of south latitude at the end of the Roman.

Your observation of Decemb 12 by your last correction is become much more agreable to the phænomenon of the tail then before & yet I fear is not altogether right. I suspect (if you are sure there was no error committed in taking its distance from Venus) that Venus had some minutes more longitude then in your recconning. In yo^{r} observations last sent the Comet Ian 10 is put in ♈ 20^{g} 42′, in a former copy in ♈ 20^{d} 49′½. That of ♈ 20^{d} 49′½ seems to correspond best with your other observations. The Parisian observations compared wth yo^{r} seem to have too much longitude. Namely Dec 29 by about 6′, Ian 4 & 6 by about 3 or 4′, Ian 8 by 7′ or 8′, Ian 13 by 12′. The greatest difference being in Ian 13, it may perhaps be worth yo^{r} while to examine yo^{r} own observation of that day before you publish it. I made an observation about that time w^{ch} though inferior in accurateness to either of yours yet may possibly give some light into the difference between you. Namely Ian. 11 I observed at half an hour past 7, that the comet, the first of the three starrs in the mouth of the north fish noted σ by Bayerus & the bright * in the north horn of ♈ were in a right line, & so were the comet & the bright stars in the shoulder & head of Andromeda. At half an hour past 8 the said star in the mouth of the fish was distant about 12′½ or 13′ from the Comet, & that distance subtended an equicrural rectanglar triangle whose equal sides comprehending the right angle were the line w^{ch} the Comet described by its motion & a perpendicular let fall from the said star in the Fishes mouth on that line. Whence the sides of that triangle were about 9′, as I also judged by observation. At nine a clock the Comet & said starr in the fishes mouth had equal altitude. At half an hour past 10, a perpendicular let fall from that star on the line described by the Comet, fell on the Comet. Whence the Comet was then in its least distance from the starr w^{ch} distance by observation was about 9′. These distances I measured only by viewing the Comet & starr through a three foot perspective w^{ch} magnified much & comparing their distance to the whole space the perspective took in. For at that time I had no better way, though afterwards when the Comet grew dimmer I fitted a 7 foot Tube wth a screw compas, by w^{ch} I traced it till March the 9th. If the place of the said star in the fishes mouth were accurately known (as it may be a month or two hence) the place of the Comet in the line of these observations would readily be given. And I think in the observations I erred not a minute, at most not 2′, but whether it be worth the while for adjusting that little difference between you & the Frenchmen to observe the place of the star I know not. S^{r} with my repeated thanks to you for yo^{r} kind communications I rest

Yo^{r} affectionate Friend
& Servant Is. NEWTON.

Will. Browne

M^{r} Paget was not Master till Apr. 1682
therefore this wrote about 85 or Ian 85$_{6}$.*

S^{r}

Whilst I was concerned that you should be so long without the sight of those papers I received a letter from M^{r} Paget by w^{ch} I understood he has been laid up sick of an ague. I am writing to him to transmit the papers to you as soon as he has a convenient opportunity. In my last I made an allowance for the distance of Iupiter & Saturn one from another diminishing their virtue in a duplicate proportion of the distance. But yet I spake there but at randome not knowing their virtues till I had your numbers for Iupiter, by which I understand his virtue is less than I supposed. But I am still at a loss for Saturn. I have not at all minded Astronomy of some years till on this occasion w^{ch} makes me more to seek. I cannot meet with Hygen's book of Saturn. Mercator & another or two w^{ch} I have consulted leave me as wise as I was. I find Saturns ring is to his body in breadth as 9 to 4 & Hygens makes the ring in Saturns nearest distance 68″ long at most, that is in his meane distance from the Sun about 1′. But it is the dimension of the orbit of the Satelles about him that I want. Now I am upon this subject, I would gladly know the bottom of it before I publish my papers. I believe you can tell me what Hygenius measures are, or if there have been any other since assigned more exactly. For by Hygenius large measures of ♃ I suspect he may have assigned the apparent diameter of the ring of ♄ too large. Your information about the error of Keplers tables for ♃ & ♄ has eased me of several scruples. I was apt to suspect there might be some cause or other unknown to me, w^{ch} might disturb the sesquialtera proportion. For the influences of the planets one upon another seemed not great enough though I imagined ♃'s influence greater then your numbers determine it. It would add to my satisfaction if you would be pleased to let me know the long diameters of the orbits of ♃ & ♄ assigned by yourself & M^{r} Halley in your new tables, that I may see how the sesquiplicate proportion fill the heavens together with another small proportion w^{ch} must be allowed for. I thank you for your kind offer of calculating the places of the Comet from the French observations to the days you mentioned. I do intend to determine the lines described by the Comets of 1664 & 1680 according to the principles of motion observed by the Planets, & should be glad of your help as to those places of the latter, if I shal not give you too much trouble. S^{r} I am

---

* Flamsteed scripsit.

Y^{or} most obliged Friend to serve you

I. NEWTON.

S^{r}

I thank you heartily for your kind information about those things I desired. In my observations about the Comet I was only carefull of the proportions of the distances from small stars to those between the starrs. Afterwards I beleive I made some mistake in the reduction to minutes & seconds, for the obseruations in many cross distances fitted[*] one another well. Your information about the Satellits of Iupiter gives me very much satisfaction. The orbit of Saturn is defined by Kepler to little for the sesquialterate proportion. This planet so oft as he is in conjunction with Iupiter ought (by reason of Iupiters action upon him to run beyond his orbit about one or two of the suns semidiameters or a little more & almost all the rest of his motion to run as much or more within it. Perhaps that might be the ground of Keplers defining it too little. But I would gladly know if you ever observed Saturn to err considerably from Keplers tables about the time of his conjunction with Iupiter. The greatest error I conceive should be either the yeare before conjunction when Saturn is 3 or 4 signes from the Sun in consequentia or the yeare after when Saturn is as far from the sun in antecedentia. You seem to insinuate as if Saturn had not yet any more satellits that one discovered by Hugenius. I should be glad to know if it be so. And one more favour I would beg, y^{t} if you have any observation of the greatest elongation of any of the satellites of Iupiter from his center (chiefly that of the outmost satellite) in degrees & minutes you would communicate it to me with the time of the observation. For I would gladly know the proportion of the orbits of the satellites to y^{t} of Iupiter as exactly as I can. And if you have any such observation of the greatest elongation of the satellite of ♄ I would also beg the favour of that. S^{r} I am

Your much obliged friend
to serve you

Is. NEWTON.

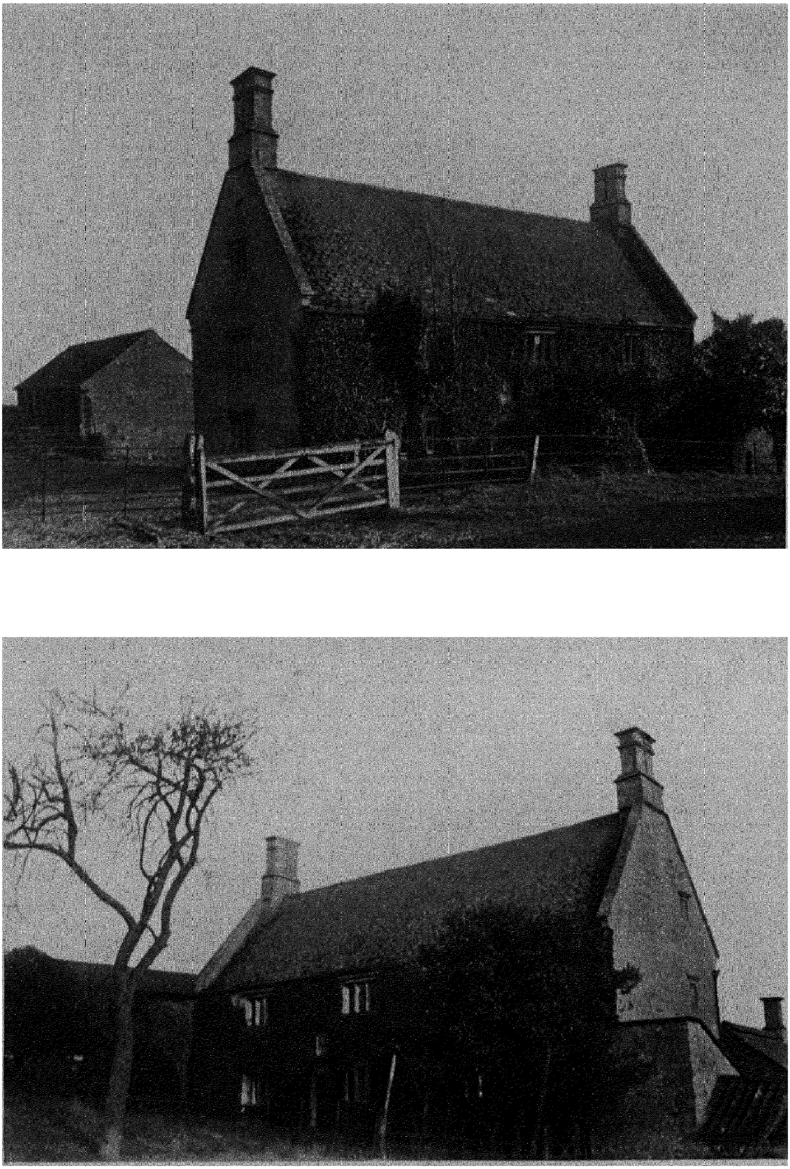
Trin. Coll. Dec 30
1680 [should be 1686 or 1685].[†]
A good new yeare
to you.

Flamsteed writes in the margin:—"Sent him hereon the elongations of all & diameters which he has employed in his Principia." *B.B. loc. cit.*

---

[*] Possibly "filled" as in *B.B* p 3228.

[†] Flamsteed's correction.



THE MANOR HOUSE, WOOLSTHORPE-BY-COLSTERWORTH.

NEWTON AND INTERPOLATION.

By Duncan C. Fraser, M.A.

The term interpolation was employed by Newton in the widest possible sense, but for the present purpose, its meaning will be restricted to interpolation by means of the formulas of finite differences. Newton gave a greater amount of attention and labour to the subject, and investigated its application in much greater detail than has hitherto been appreciated, and some account will be given of his unpublished as well as published work.

In the second of the autobiographical letters which Newton wrote in 1676 to Oldenburg, the Secretary of the Royal Society (for the information of Leibnitz) on the work he had done in connection with the development of functions of x in terms of a series of powers of x he makes the remark :—

"When simple series are not obtainable with sufficient ease I have another method not yet published by which the problem is easily dealt with. It is based upon a convenient, ready, and general solution of the problem. *To describe a geometrical curve which shall pass through any given points. . . .*"

"Although the problem may seem to be intractable at first sight it is nevertheless quite the contrary. Perhaps indeed it is one of the prettiest problems that I can ever hope to solve."

A complete solution of this problem is given in Lemma 5, Book 3, of the *Principia*, and this solution has always been recognised as the fundamental proposition of the Calculus of finite differences. It is enunciated by Newton without any indication of the method by which he obtained the result. Another solution was given by Newton in the *Methodus Differentialis*, a little treatise of nine quarto pages in which he gives several propositions with their proofs. This was first published in the year 1711 by William Jones in a volume in which he collected a number of Newton's shorter works under the title *Analysis per Quantitatum Series, Fluxiones ac Differentias ; cum Enumeratione Linearum Tertii Ordinis.*

Although the *Methodus Differentialis* is of first class importance in the history of interpolation formulas it has been to a remarkable extent disregarded, perhaps because until recently it has been available only in the original Latin. In October, 1918, however, a reproduction and a translation of the little tract were published in the *Journal* of the Institute of Actuaries.

In a preface William Jones says : "The *Methodus Differentialis* which I have transcribed by permission of the distinguished author from his own autograph . . . depends upon the problem of drawing a parabolic curve through a given number of points, reference to which had been made by the distinguished author in his letter to Oldenburg, sent in 1676, and a solution of which he gave in Lemma 5, Book 3, of his *Principia* by means of a construction not at all the same as that which we now present."

There is another interesting question arising out of the second letter to Oldenburg. In giving directions for the construction of Logarithmic Tables Newton explains a simple process of obtaining the Logarithms of the numbers 98, 99, 100, 101, 102, and then he says :

"These are to be interpolated by intervals of one tenth (Hi per dena intervalla interpolandi sunt), and the Logarithms of all numbers between 980 and

1020 being again interpolated by intervals of one tenth the table will so far be constructed; then from these are to be collected the Logarithms of all the primes less than 100 and their multiples."

After giving detailed instructions for calculating such primes he goes on:

"And so having found the Logarithms of all numbers less than 100, it remains to interpolate these also once and again by intervals of one tenth."

He then discusses the construction of Trigonometrical tables and says finally:

"What has been said about tables of this kind can be applied to others; . . . it is sufficient by means of these series to calculate 30 or 20 or even fewer terms at suitable distances apart, since the intermediate terms are easily inserted by a method which I had almost decided to describe here for the use of computers."

The only description of a method of interpolation by intervals of one tenth which has up to the present time been available is contained in a letter from Newton to Mr. John Smith dated 8th May, 1675, published by Mr. J. Edleston in 1850 in a volume entitled *Correspondence of Sir Isaac Newton and Professor Cotes, including letters of other eminent men, etc.* The letter to John Smith deals with the simple case of the computation of tables of square, cube, and fourth roots.

There is, however, a Latin manuscript in the Portsmouth Collection in the University Library in Cambridge entitled *Regula Differentiarum*, which upon examination turns out to be a draft of the matter which, in his letter to Oldenburg, Newton says: "I had almost decided to describe here for the use of computers."

The following sentence translated from the manuscript establishes the connection:

"I shall not hesitate to give more fully here the explanation of the formula which most pleases me though . . . it is not of so much importance that a detailed account of it should be given to Leibnitz."

The *Regula Differentiarum* is incomplete, but it contains a full description of interpolation by intervals of one tenth by a method differing from that described in the letter to John Smith, and it includes a new and most interesting statement of the general fundamental proposition on which the formulae of interpolation are based.

The material which is available for examination therefore comprises the following items:

(1) The *Methodus Differentialis.* From the dates mentioned by Newton in the letter to Oldenburg, it appears to be probable that this was written quite early in the period 1665-1671. It was not published until 1711.

(2) *Lemma* 5 in Book 3 of the *Principia,* written and published in 1686.

(3) The *Letter to John Smith,* written and sent to John Smith in 1675, sent by William Jones to Professor Cotes in 1712, printed by Mr. Edleston in 1850.

(4) The *Regula Differentiarum,* written in 1676, not hitherto published. It is, however, now in course of publication by the Institute of Actuaries.

METHODUS DIFFERENTIALIS.

In several propositions of this work he employs divided differences according to a scheme which (not using his notation) may be expressed as follows:

Value of Independent Variable.	Value of Function.	1st divided differences.	2nd divided differences.	3rd divided differences.	4th divided differences.
a	Ua				
		$\dfrac{Ua-Ub}{a-b}$ $=\grave{\Delta}(ab)$			
b	Ub		$\dfrac{\grave{\Delta}(ab)-\grave{\Delta}(bc)}{a-c}$ $=\grave{\Delta}^2(abc)$		
		$\dfrac{Ub-Uc}{b-c}$ $=\grave{\Delta}(bc)$		$\dfrac{\grave{\Delta}^2(abc)-\grave{\Delta}^2(bcd)}{a-d}$ $=\grave{\Delta}^3(abcd)$	
c	Uc		$\dfrac{\grave{\Delta}(bc)-\grave{\Delta}(cd)}{b-d}$ $=\grave{\Delta}^2(bcd)$		$\dfrac{\grave{\Delta}^3(abcd)-\grave{\Delta}^3(bcde)}{a-e}$ $=\grave{\Delta}^4(abcde)$
		$\dfrac{Uc-Ud}{c-d}$ $=\grave{\Delta}(cd)$		$\dfrac{\grave{\Delta}^2(bcd)-\grave{\Delta}^2(cde)}{b-e}$ $=\grave{\Delta}^3(bcde)$	
d	Ud		$\dfrac{\grave{\Delta}(cd)-\grave{\Delta}(de)}{d-e}$ $=\grave{\Delta}^2(cde)$		
		$\dfrac{Ud-Ue}{d-e}$ $=\grave{\Delta}(de)$			
e	Ue				

Proposition 1 shows how to obtain expressions for the divided differences. It is assumed that the ordinate of a curve at the point $A+x$ can be represented by the expression $a+bx+cx^2+dx^3+ex^4$, and in a table which explains itself and is reproduced below (with the substitution of English headings for the Latin of the original), he sets out at full length the values of the ordinates, and of their divided differences when x has the values p, q, r. s, t.

Abscissa.	Ordinates.	
$A+p$	$a+bp+cp^2+dp^3+ep^4$	$=\alpha$
$A+q$	$a+bq+cq^2+dq^3+eq^4$	$=\beta$
$A+r$	$a+br+cr^2+dr^3+er^4$	$=\gamma$
$A+s$	$a+bs+cs^2+ds^3+es^4$	$=\delta$
$A+t$	$a+bt+ct^2+dt^3+et^4$	$=\epsilon$

Divisor.	Difference.	Quotients.
$p-q$	$\alpha-\beta$	$b+c(p+q)+d(p^2+pq+q^2)+e(p^3+p^2q+pq^2+q^3)=\zeta$
$q-r$	$\beta-\gamma$	$b+c(q+r)+d(q^2+qr+r^2)+e(q^3+q^2r+qr^2+r^3)=\eta$
$r-s$	$\gamma-\delta$	$b+c(r+s)+d(r^2+rs+s^2)+e(r^3+r^2s+rs^2+s^3)=\theta$
$s-t$	$\delta-\epsilon$	$b+c(s+t)+d(s^2+st+t^2)+e(s^3+s^2t+st^2+t^3)=\kappa$
$p-r$	$\zeta-\eta$	$c+d(p+q+r)+e(p^2+pq+q^2+pr+qr+r^2)\quad=\lambda$
$q-s$	$\eta-\theta$	$c+d(q+r+s)+e(q^2+qr+r^2+qs+rs+s^2)\quad=\mu$
$r-t$	$\theta-\kappa$	$c+d(r+s+t)+e(r^2+rs+s^2+rt+st+t^2)\quad=\nu$
$p-s$	$\lambda-\mu$	$d+e(p+q+r+s)\quad=\xi$
$q-t$	$\mu-\nu$	$d+e(q+r+s+t)\quad=\pi$
$p-t$	$\xi-\tau$	$e\quad=\sigma$

Proposition 2 points out that the last divided difference is equal to the last coefficient e; and that if the values of the ordinates $\alpha\beta\gamma\delta\epsilon$ are known corresponding to values p, q, r, s, t, of x, then the equation of a parabolic curve which passes through the extremities of the given ordinates can be obtained. The last divided difference σ gives the coefficient of the last term e; and the coefficients of the remaining terms can easily be derived in succession. Thus $d=\xi-e(p+q+r+s)$; $c=\lambda-d(p+q+r)-e(p^2+pq+q^2+pr+qr+r^2)$; etc.

He gives detailed instructions in the same manner for the formation of each coefficient, but does not proceed to give the form of the result. If, however, we follow his instructions precisely and express the coefficients in terms of the leading ordinate and the leading differences, the resulting formula for the ordinate at the point whose abscissa is $A+x$ is

$$\alpha+\zeta(x-p)+\lambda(x-p)(x-q)$$
$$+\xi(x-p)(x-q)(x-r)+\sigma(x-p)(x-q)(x-r)(x-s).$$

Here α is the ordinate at the point $A+p$, and may be called U_p; ζ is the first difference corresponding to the points $A+p$ and $A+q$ and may be called $\grave{\Delta}(pq)$; λ is the second difference corresponding to the points $A+p$, $A+q$, $A+r$, and may be called $\grave{\Delta}^2(pqr)$, and so on.

Thus the ordinate U_x at the point $A+x$ is given by the formula

$$U_x=U_p+(x-p)\grave{\Delta}(pq)+(x-p)(x-q)\grave{\Delta}^2(pqr)$$
$$+(x-p)(x-q)(x-r)\grave{\Delta}^3(pqrs)+(x-p)(x-q)(x-r)(x-s)\grave{\Delta}^4(pqrst).$$

If we put $p=0$, $q=1$, $r=2$, $s=3$, $t=4$, we have the case of ordinates separated by intervals all equal to unity, and the ordinate at a distance x from the initial ordinate is:

$$U_x=U_0+x\grave{\Delta}(0\,.\,1)+x(x-1)\,\grave{\Delta}^2(0\,.\,1\,.\,2)$$
$$+x(x-1)(x-2)\grave{\Delta}^3(0\,.\,1\,.\,2\,.\,3)+x(x-1)(x-2)(x-3)\grave{\Delta}^4(0\,.\,1\,.\,2\,.\,3\,.\,4).$$

In this formula the differences are still divided differences. Expressed in terms of ordinary differences :

$$\grave{\Delta}\,(0\,.\,1) \text{ is } \Delta U_0, \qquad \grave{\Delta}^3(0\,.\,1\,.\,2\,.\,3) \text{ is } \tfrac{1}{6}\Delta^3 U_0,$$
$$\grave{\Delta}^2(0\,.\,1\,.\,2) \text{ is } \tfrac{1}{2}\Delta^2 U_0, \qquad \grave{\Delta}^4(0\,.\,1\,.\,2\,.\,3\,.\,4) \text{ is } \tfrac{1}{24}\Delta^4 U_0.$$

Using ordinary differences the formula therefore becomes :

$$U_x = U_0 + x\Delta U_0 + x_2\Delta^2 U_0 + x_3\Delta^3 U_0 + x_4\Delta U_0,$$

where we write x_2 for $\frac{x(x-1)}{2}$, x_3 for $\frac{x(x-1)(x-2)}{6}$, etc.

This is the ordinary Binomial formula. It is obtained by following Newton's instructions, but he does not give the form of the result.

From these propositions, he remarks, those which follow are easily inferred ; and accordingly he does not give detailed proofs, but describes the data and enunciates the results.

Proposition 3 deals with the question of finding the parabolic curve which shall pass through extremities of ordinates standing at equal distances from one another, the equal distances being taken as unity. The cases of odd and even numbers of ordinates are taken separately.

It should be mentioned that he departs from the principle of Proposition 1, by taking ordinary undivided differences instead of divided differences; but the algebraical work is much easier with divided differences, and I give a full proof of one case to illustrate this.

Case 1. When the number of ordinates is odd, distances are measured from the foot of the central ordinate, and the values employed are those of the central ordinate and of the central differences. The differences are set out in the form of a triangular diagram accompanied by a geometrical figure, as below:

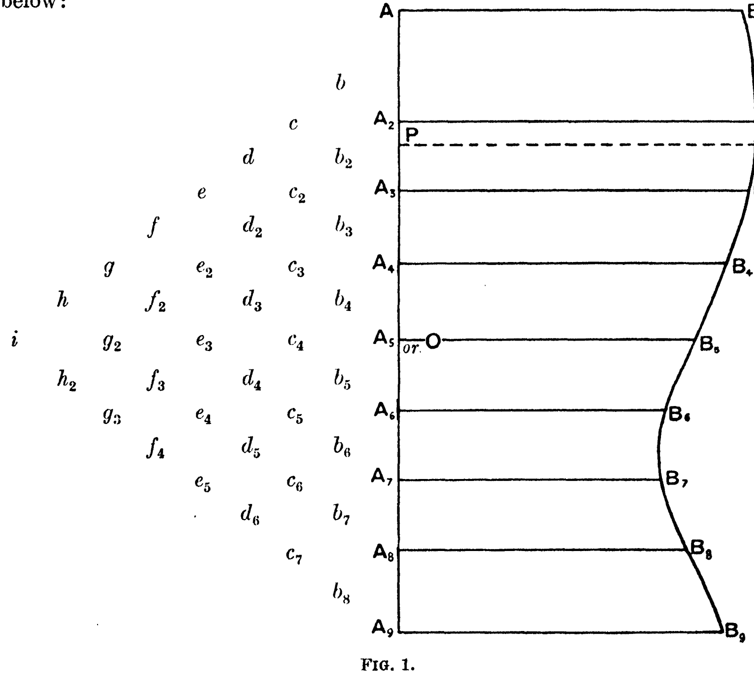


FIG. 1.

It will be noticed that the diagram of differences is left-handed, while we are now accustomed to use a right-handed diagram. He takes the differences in the sense opposite to modern practice and subtracts the lower from the upper values, and as he also measures the values of x in a sense opposite to modern practice the form of the result is not affected.

The values used are denoted by the letters k, l, m, n, o, p, q, r, where k is the ordinate at A or O, being the central ordinate; l, n, p, r, are the mean odd central differences, so that $l=\frac{1}{2}(b_4+b_5)$, etc.; and m, o, q, are the even central differences, c_4, e_3, g_2, and i.

For the purposes of proof, however, take the divided differences l', m', n', o', p', q', r', where $l'=l$, $2m'=m$, $6n'=n$, $24o'=o$, $120p'=p$, $720q'=q$.

Following exactly the process of Proposition 1, and assuming that the ordinate at x is represented by the expression

$$A+Bx+Cx^2+Dx^3+Ex^4+Fx^5+Gx^6$$

it is easily found that:

$$\begin{aligned}
A&=k,\\
B+D+F&=l' &&=l,\\
C+E+G&=m' &&=2\times m,\\
D+5F&=n' &&=6\times n,\\
E+5G&=o' &&=24\times o,\\
F&=p' &&=120\times p,\\
G&=q' &&=720\times q.
\end{aligned}$$

It follows at once that the value of the ordinate at x is:

$$\begin{aligned}
&k+(l'-n'+4p')x+(m'-o'+4q')x^2+(n'-5p')x^3+(o'-5q')x^4+p'x^5+q'x^6\\
&=k+l'x+m'x^2+n'x(x^2-1)+o'x^2(x^2-1)+p'x(x^2-1)(x^2-4)\\
&\qquad+q'x^2(x^2-1)(x^2-4)\\
&=k+xl+\frac{x^2}{2}m+\frac{x(x^2-1)}{6}n+\frac{x^2(x^2-1)}{24}o+\frac{x(x^2-1)(x^2-4)}{120}p\\
&\qquad+\frac{x^2(x^2-1)(x^2-4)}{720}q.
\end{aligned}$$

The expression of the result as given by Newton is:

$$k+xl+\frac{x^2}{2}m+\frac{x^3-x}{6}n+\frac{x^4-x^2}{24}o+\frac{x^5-5x^3+4x}{120}p, \text{ etc.}$$

Case 2. When the number of ordinates is even, the ordinates at A_4 and A_5 are made the central ordinates and the origin is taken midway between them. k, l, m, n, o, p, q, represent the values of the mean central ordinate and of the central differences in line with the origin, the even central differences being mean values. The resulting formula is:

$$k+xl+\frac{4x^2-1}{8}m+\frac{x(4x^2-1)}{24}n+\frac{(4x^2-1)(4x^2-9)}{120}o+\text{etc.}$$

Proposition 4 gives the corresponding formulas when the values between the ordinates are not equal. He follows strictly the procedure of Proposition 1, and applies it to obtain central difference formulas for the two cases when the number of given values is odd and when the number is even.

Proposition 5 merely points out that the method can be applied to find an approximate value for any intermediate term.

Proposition 6 mentions that the formulas may be applied to obtain the approximate area of any curve a number of whose ordinates can be ascertained.

In the Scholium he remarks that these propositions are useful for the construction of tables by interpolation, as also for the solution of problems which depend on finding the areas of curves, especially if the intervals between the ordinates are small and equal to one another.

The formulas given in the two cases of Proposition 3 are usually described as Stirling's formula and Bessel's formula respectively. They are Central Difference formulas, and are given so clearly, in algebraic form, that any student familiar with modern books on the subject at once recognizes Newton's statement of them.

In Propositions 1 and 2, which together constitute the fundamental proposition, he gives full instructions for obtaining the algebraic result, but deliberately refrains from giving it expression. Following out his directions the result has been obtained and given above. It may be conjectured that his mind was so bent on giving prominence to those propositions that are of direct use for the calculation of tables, that he suppressed the statement of the general result. It was, there can be no doubt, from the consideration of methods suitable for the calculation of tables that he was led to the consideration of formulas of interpolation, and the central difference formulas are of primary importance for this purpose.

LETTER TO JOHN SMITH, DATED 8TH MAY, 1675.

The letter gives directions for computing tables of square roots, cube roots, and fourth roots of numbers. It will be sufficient to explain the method of obtaining the table of square roots.

First of all the square root of every 100th number is obtained by an application of the binomial theorem.

(1) In the scheme below, F is a number whose square root has been calculated; the first, second and third central differences s, st, and m are obtained from properties of the function, and the value of every tenth square root is obtained by completing the scheme on the assumption of constant third differences, the column of second differences being completed to the extent of five steps in each direction, then the column of first differences, and then the column of the values of the function.

Number.	Square Root.	First Differences.	Second Differences.	Third Differences.
$n-50$	A			
		o		
$n-40$	B		op	
		p		
$n-30$	C		pq	
		q		
$n-20$	D		qr	
		r		
$n-10$	E		rs	
		s		m
n	F		st	
		t		m
$n+10$	G		tv	
		v		
$n+20$	H	etc.		

Here the differences have reference to intervals of ten units,

$$i.e.\quad s=\sqrt{n}-\sqrt{n-10}.$$

The equations for s, st, and m are:

$$s=\left(\frac{10}{2n}\right)F+\tfrac{1}{2}st+\tfrac{1}{6}m,$$

$$st=\left(\frac{10}{2n}\right)^2F,$$

$$m=3\left(\frac{10}{2n}\right)^3F.$$

(2) Now consider the following scheme:

Number.	First Differences.	Second Differences.
$n-50$	α	
		o
$n-40$	β	
		υ
$n-30$	γ	
		χ
$n-10$	δ	
		ρ
n	ϵ	
		σ
$n+10$	ζ	
		τ
$n+20$	η	

Here the differences have reference to intervals of one unit,

$$i.e.\quad \epsilon=\sqrt{n}-\sqrt{n-1}.$$

By an application of Stirling's Theorem, he finds:

$$\zeta=\frac{F}{2n}+\frac{st}{200}.$$

$$\sigma=\frac{st}{10}+\frac{55m}{1000}.$$

From these values the column of second differences is completed in each direction from σ on the basis of the constancy of m, then the column of first differences is completed.

(3) Every tenth value of the function being known from (1) and every tenth first difference from (2), the column of first differences is completed on the basis of constant second differences $=\frac{st}{100}$, changing with ten steps to $\frac{tr}{100}$, etc., and then the column of values of the function is completed by "Addition and Subduction."

PRINCIPIA, BOOK 3, LEMMA 5.

The proposition given in the Lemma has always been recognized as the foundation of the theory of interpolation by means of formulas of Finite Differences. It was Newton's first printed contribution to the subject and is the only one of his contributions with which most mathematicians are acquainted. It gives without demonstration the result which Newton had demonstrated but had refrained from expressing in Proposition 2 of the *Methodus*.

As the Lemma is of so much importance in the theory of the subject I give a literal translation in full.

To find a parabolic curve which shall pass through any given points.

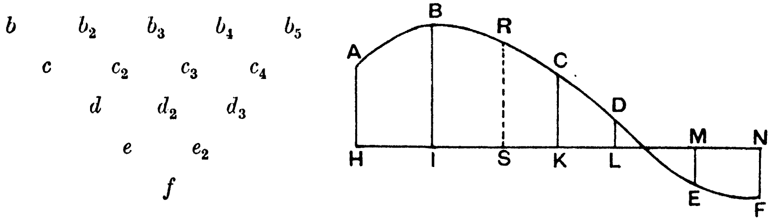


FIG. 2.

Let the points be A, B, C, D, E, F, etc., and from them let fall perpendiculars AH, BI, CK, DL, EM, FN, on any given straight line HN.

Case 1. If the intervals HI, IK, KL, etc., between the points H, I, K, L, M, N, are equal, find the first differences b, b_2, b_3, b_4, b_5, etc., of the perpendiculars ; the second differences c, c_2, c_3, c_4, etc., the third differences d, d_2, d_3, etc., so that $AH-BI=b$, $BI-CK=b_2$, $CK-DL=b_3$, $DL+EM=b_4$, $EM+FM=b_5$, etc., $b-b_2=c$, etc., and so on to the last difference, which in this case is f. Then having erected any perpendicular RS, which shall be an ordinate to the required curve, in order to find its length put each of the intervals HI, IK, KL, LM, equal to unity, and let $Ah=a$, $-HS=p$, $\frac{1}{2}p(-IS)=q$, $\frac{1}{3}q\times SK=r$, $\frac{1}{4}r\times SL=s$, $\frac{1}{5}s\times SM=t$, proceeding in the same way to the last but one of the perpendiculars ME, and prefixing the negative sign to the terms HS, IS, etc., which lie on the same side of the point as A, and the positive signs to the terms SK, SL, etc., which lie on the other side of the point S. Then observing the correct signs, RS will be

$$a+bp+cq+dr+es+ft, \text{ etc.}$$

Case 2. But if the intervals HI, IK, etc., between the points H, I, K, L, are unequal find b, b_2, b_3, b_4, b_5, being the first differences of the perpendiculars AH, BI, CK, etc., divided by the intervals between the perpendiculars ; c, c_2, c_3, c_4, etc., the second differences divided by the differences between the alternate perpendiculars ; d, d_2, d_3, the third differences divided by the intervals between every fourth perpendicular, and so on ; so that $b=\frac{AH-BI}{HI}$, $b_2=\frac{BI-CK}{IK}$, $b_3=\frac{CK-DL}{KL}$, etc., $c=\frac{b-b_2}{HK}$, $c_2=\frac{b_2-b_3}{IL}$, $c_3=\frac{b_3-b_4}{KM}$, etc., $d=\frac{c-c_2}{HL}$, $d_2=\frac{c_2-c_3}{IM}$. The differences having been ascertained, put $AH=a$, $-HS=p$, $p\times(-IS)=q$, $q\times SK=r$, $r\times SL=s$, $s\times SM=t$, proceeding, it will be understood, to the last perpendicular but one, ME, then the ordinate RS will be

$$=a+bp+cq+dr+es+ft, \text{ etc.}$$

Corollary. Hence the areas of all curves can be ascertained approximately. For if several points are found of the curve whose area is required and a parabolic curve is drawn through these points, its area will be approximately the same as that of the given curve. But the area of the parabolic curve can always be found geometrically by methods that are very well known.

It will be noticed that he takes the differences in a sense opposite to modern practice, and that he also reverses the signs of the abscissae as compared with the modern convention.

If in Case 1 we call the ordinates U_0, U_1, U_2, etc., and take $HS=x$, then since $HI=IK=KL=\text{etc.}=1$, $IS=x-1$, $SK=x-2$, $SL=x-3$, $SM=x-4$, $SN=x-5$. Here values of the abscissa are positive where Newton makes them negative, and negative where he makes them positive.

On the other hand, we make $U_1-U_0=\Delta U_0$, while he makes the first difference of $U_0=U_0-U_1$, and similarly in other cases. Substituting this notation for Newton's the formula for U_x becomes :

$$U_x=U_0+x\,\Delta U_0+\frac{x(x-1)}{2}\,\Delta^2 U_0+x\frac{(x-1)(x-2)}{6}\,\Delta^3 U_0$$

$$+x\frac{(x-1)(x-2)(x-3)}{24}\,\Delta^4 U_0+x\frac{(x-1)(x-2)(x-3)(x-4)}{120}\,\Delta^5 U_0,\ \text{etc.}$$

In Case 2, if we call the perpendiculars U_p, U_q, U_r, U_s, U_t, where the suffixes denote the values of x at the points where they are erected, then the formula for Ux can be written :

$$\begin{aligned}U_x=U_p&+(x-p)\grave{\Delta}(pq)\\&+(x-p)(x-q)\grave{\Delta}^2(pqr)\\&+(x-p)(x-q)(x-r)\grave{\Delta}^3(pqrs)\\&+(x-p)(x-q)(x-r)(x-s)\grave{\Delta}^4(pqrst).\end{aligned}$$

These results are the same as those obtained by following Newton's directions given in Proposition 2 of the *Methodus.*

REGULA DIFFERENTIARUM.

The MS. is on two separate sheets of foolscap. The first sheet contains directions for the calculation of Tables of Sines and of the Logarithms of the natural numbers, and repeats matter of the same nature which appears in a different order and with slight variations of language in the second letter to Oldenburg. After the section on Logarithms he goes on :

"The process of supplying the missing terms of any Table when first differences suffice, as is commonly done in Logarithmic Tables, is obvious and familiar. But when second, third, and other differences have to be considered, there seems to be a want of a general proposition giving the best method of arranging and combining these differences. I shall not hesitate to give more fully here the explanation of the formula which most pleases me though it is hardly suitable for this place, and it is not of so much importance that a detailed account of it should be given to Leibnitz, but since it is applicable to the calculation of Tables of every kind, you may communicate it to computers if you know of any, and if it seems likely to be of service to them."

He then gives rules for seven cases which may be summarised as follows :

Rules I, II, and III merely refer to detailed instructions which are given later on in the MS. for the respective cases of (1) uniform intervals with 2nd differences constant ; (2) irregular intervals with 2nd differences constant ; and (3) uniform intervals with 3rd differences constant.

Rule IV gives as the formula for the insertion of a mean term among four given terms A, B, C, and D,

$$\frac{9(B+C)-(A+D)}{16}$$

with an error $=\frac{3}{128}$ of the 4th adjusted difference.

Rule V gives as the formulas for the insertion of two mean terms among the same ordinates,

$$\frac{60B+30C-5A-4D}{81}$$

and

$$\frac{60C+30B-5D-4A}{81},$$

with errors $= \frac{5}{243}$ of the 4th difference.

Rule VI gives a formula for the insertion of a mean term among six terms A, B, C, D, E, F, namely:

$$\frac{150R-25S+3T}{256},$$

where

$$R=C+D, \quad S=B+E, \quad T=A+F,$$

with an error less than $\frac{1}{200}$th part of the 5th difference.

Rule VII contains an interesting discussion of the question of inverse interpolation, and this sheet of the MS. ends with the paragraph :

" I derive these rules from the consideration of our infinite series by supposing the terms of a table to be expressed by means of such series, for example

$$a+bx+cx^2+dx^3, \text{ etc.},$$

where the indefinite quantity x denotes the difference between the number corresponding to a term and any other number very near to it, and

$$bx, \; cx^2, \; dx^3, \text{ etc.},$$

are quantities from which the first, second, and third differences are derived. But about these matters I have said more than enough."

The first sheet of the MS. which we have described above is not headed with any title, but the heading " Regula Differentiarum " appears on the second sheet, which begins with an elaborate explanation of the process of interpolation by intervals of one tenth.

The question is, to find the simplest way of obtaining U_0, U_1, U_2, U_3, etc., when we know $U_0, U_{10}, U_{20} \ldots$.

It may be observed at the outset that he uses divided differences according to an adjusted scheme. Taking uniform intervals of n, the divided differences of any order are obtained from those of the preceding order by taking differences and dividing always by n, while according to the scheme of the *Methodus* and of the *Lemma*, the divisors employed in obtaining first, second, third, and fourth differences, etc., respectively would be n, $2n$, $3n$, $4n$, etc. When n becomes unity the adjusted divided differences employed in the *Regula* become the ordinary differences, which are obtained by continual differencing without any division. There is, therefore, a convenient relation between divided differences of the adjusted scheme and ordinary differences.

For example when the fourth differences are small enough to be neglected the adjusted third differences of the series U_0, U_n, U_{2n}, etc., are the same as the ordinary third differences of the series U_0, U_1, U_2, etc., and the first step towards obtaining the required values U_1, U_2, etc., is made without trouble.

Turning now to Newton's explanation of his process of interpolation by intervals of one tenth, he gives a table as below, in which A, B, C, are values of the function separated by ten intervals.

$K_1 = \frac{A-B}{10}, \quad K_2 = \frac{B-C}{10}$, etc., are adjusted first differences.

$L_1 = \frac{K_1-K_2}{10}, \quad L_2 = \frac{K_2-K_3}{10}$ are adjusted second differences.

These differences are set out on the left of the table.

$$\begin{array}{cccc}
L_1=l_1 & A & & t_1 \\
 & & p_1 & \\
 & a_1 & & t_2 \\
 & & p_2 & \\
 & a_2 & & t_3 \\
 & & p_3 & \\
 & a_3 & & t_4 \\
 & & p_4 & \\
 & a_4 & & t_5 \\
 & & p_5 & \\
 & a_5 & & t_6 \\
K_1 & & p_6 & \\
 & a_6 & & t_7 \\
 & & p_7 & \\
 & a_7 & & t_8 \\
 & & p_8 & \\
 & a_8 & & t_9 \\
 & & p_9 & \\
 & a_9 & & t_{10} \\
 & & p_{10} & \\
L_2=l_2 & B & & v_1 \\
 & & q_1 & \\
 & b_1 & & v_2 \\
 & & q_2 & \\
 & b_2 & & v_3 \\
 & & q_3 & \\
 & b_3 & & v_4 \\
 & & q_4 & \\
 & b_4 & & v_5 \\
 & & q_5 & \\
 & b_5 & & v_6 \\
K_2 & & q_6 & \\
 & b_6 & & \\
 & \vdots & & \\
L_3=l_3 & C & &
\end{array}$$

The small letters a_1, a_2, a_3, etc., represent the required interpolated values ; p_1, p_2, p_3, etc., are their first differences, and $t_1, t_2, t_3, \ldots\ v_1, v_2, v_3, \ldots$ are second differences. These are ordinary differences.

He assumes first of all that third differences of the values A, B, C, etc., can be neglected.

L_1, L_2, L_3 are put $=l_1$, l_2, l_3, the small letters indicating that they are taken as second differences of the interpolated values. The second differences of the interpolated values are then settled according to the following scheme.

$$\begin{array}{l}
t_1=t_2=t_3=t_4=l_1, \\
t_5=t_6=t_7=z_1, \quad \text{where } z_1=\tfrac{1}{2}(l+l_2) \\
\left.\begin{array}{l} t_8=t_9=t_{10} \\ =v=v_2=v_3=v_4 \end{array}\right\} =l_2.
\end{array}$$

The central first differences of the interpolated values are :

$$\begin{aligned}
p_5&=K_1+\tfrac{1}{2}z_1, \\
p_6&=K_1-\tfrac{1}{2}z_1. \\
q_5&=K_2+\tfrac{1}{2}z_2, \\
q_6&=K_2-\tfrac{1}{2}z_2.
\end{aligned}$$

It is noted that if the number of intervals should be odd the central first difference would be $=K$.

Starting from p and working downwards the first differences are determined in succession down to q_1.

$$p_6 - t_7 = p_7,$$
$$p_7 - t_8 = p_8,$$
$$p_8 - t_9 = p_9,$$
$$p_9 - t_{10} = p_{10},$$
$$p_{10} - v_1 = q_1.$$

Starting from q and working upwards the first differences are determined in succession up to q_1.

$$q_5 + v_5 = q_4, \text{ etc.}$$

And the coincidence in the two values of q_1 so obtained is a check on the accuracy of the work.

The first differences being thus calculated the interpolated values are ascertained in succession from A to a_5, and from B to a_5, and the coincidence of the two values of a_5 is a check on the work.

A similar process is followed between B and C, and so each interval is filled up, checks being provided at all stages of the work.

Similar directions are given for the case when the intervals are unequal.

These arrangements are applicable, Newton remarks, when the third differences are less than 8. He refers here to the ordinary third differences, undivided, of the given terms A, B, C, D, etc. When they are greater than 8, and 4th differences are not greater that 32, he gives the following expressions for the central first differences of the required terms.

Let M be the adjusted third differences. Taking the number of intervals between the given terms to be n, when n is even there will be two central first differences between A and B, namely :

$$K + \frac{n^2 - 4}{24} M + \tfrac{1}{2}z,$$

and

$$K + \frac{n^2 - 4}{24} M - \tfrac{1}{2}z,$$

and when n is odd, there will be one central difference :

$$K + \frac{n^2 - 1}{24} M.$$

The central second differences will be unaltered and the remaining second differences will be completed by the continuous addition or subtraction of the constant 3rd differences M.

The columns of first differences can then be completed, and the interpolated values obtained in succession.

When differences beyond the 3rd differences are too great to be neglected, he directs that the values of the mean ordinates should be calculated and inserted, and that this process of bisection should be carried on until 4th and higher differences can be neglected. He then gives a rule for calculating the value of the mean ordinate when six ordinates are given.

The formulas employed above can be easily derived from Stirling's and Bessel's Formulas, as given in the *Methodus* which Newton had written several years before, but had not given to the world.

It might have been expected that he would take the opportunity of giving the propositions of the *Methodus* in order to explain the algebraic forms which he introduces into the directions of the *Regula*. Up to the time he was writing, algebraic expressions for formulas of interpolation had never been used, and they were unknown except so far as he may have disclosed them to individuals or in lectures to pupils.

The propositions of the *Methodus* must have been before him when he was compiling the *Regula*, but when he came to review them, it may be supposed that the idea came to him that the laborious algebra of the *Methodus* was quite unnecessary, and that the fundamental proposition could be presented in a new form as a self-contained scheme which verified itself.

Instead therefore of giving a number of separate propositions, he says at this stage:

"Other rules of this kind could be given but I prefer to express the whole matter in a single general rule, and to show how any series can have a term inserted in any required place."

A translation is here given of that part of the manuscript which relates to the single general rule, omitting the elaborate directions as to signs which the absence of our modern conventions made necessary, and correcting obvious errors of the pen.

NEWTON'S SINGLE GENERAL RULE.

Let a series be represented by the lines *Ap*, *Bq*, *Cr*, *Ds*, *Et*, *Fu*, *Gv*, etc., erected perpendicularly on the line *AG*, and the intervals between the terms by the parts of that line, namely *AB*, *BC*, *CD*, *DE*, etc., and so on to the end. [It will be noticed that in the expressions given below Newton writes *A* for *Ap*, *B* for *Bq*, etc.]

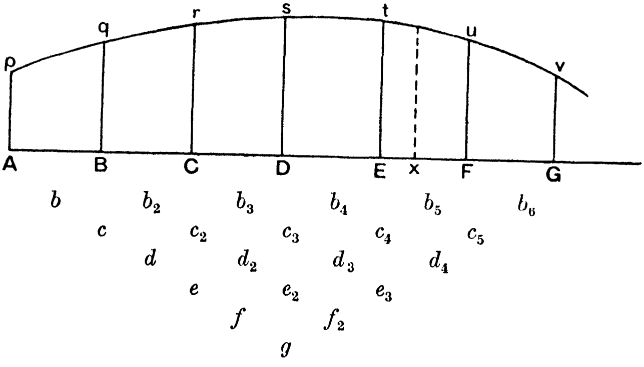


FIG. 3.

That is, make $\frac{A-B}{AB}=b$; $\frac{B-C}{BC}=b_2$; $\frac{C-D}{CD}=b_3$, etc.,

also $\frac{b-b_2}{\frac{1}{2}AC}=c$; $\frac{b_2-b_3}{\frac{1}{2}BD}=c_2$; $\frac{b_3-b_4}{\frac{1}{2}CE}=c_3$, etc.,

and then $\frac{c-c_2}{\frac{1}{3}AD}=d$; $\frac{c_2-c_3}{\frac{1}{3}BE}=d_2$; $\frac{c_3-c_4}{\frac{1}{3}CF}=d_3$, etc.,

and further $\frac{d-d_2}{\frac{1}{4}AE}=e$; $\frac{d_2-d_3}{\frac{1}{4}BF}=e_2$; $\frac{d_3-d_4}{\frac{1}{4}CG}=e_3$, etc.,

and so on in succession to the end, always dividing the first differences by the intervals of the terms of which they are the differences, the second differences by half of the two intervals to which they correspond, the third differences by one-third part of the three intervals of which they correspond, proceeding in this way until the difference in the last place is sufficiently small.

When this has been done take the leading term A and the leading differences b, c, d, e, f, g. Let the number of these differences be n, the position where the interpolation is to be made x, and the term to be interpolated xy. Then working backwards from the last difference, say g, and from the last but one of the terms from which the differences were obtained, say F, make

$$f+\frac{g\times Fx}{n}=p;\quad e-\frac{p\times Ex}{n-1}=q;$$

$$d-\frac{q\times Dx}{n-2}=r;\quad c-\frac{r\times Cx}{n-3}=s;$$

$$b-\frac{s\times Bx}{n-4}=t;\quad A-\frac{t\times Ax}{n-5}=v;$$

and so on up to the first term A; and the final result, say v, will be the interpolated term which had to be found;

or make

$$f+\frac{Fx}{n}\times g=p;\quad e-\frac{Ex}{n-1}\times p=q,$$

$$d-\frac{Dx}{n-2}\times q=r;\quad c-\frac{Cx}{n-3}\times r=s;$$

$$b-\frac{Bx}{n-4}\times s=t;\quad A-\frac{Ax}{n-5}\times t=v.$$

This rule requires no further demonstration than that the method of passing from the differences to the required term xy is the same as that by which we pass from the given terms to the differences, for the required term xy becomes identical with the given terms Ap, Bq, Cr, etc., respectively, when the point x coincides with the respective points A, B, C, etc., so that the ordinate terminates at a certain curved line pv, which passes through all the points p, q, r, s, t, u, v.

But the proposition should be illustrated by an example. Therefore let the logarithms of the numbers 24, 25, 26, 27, 28, 29, 30, be given, and let it be required to find the logarithm of any intermediate number; then this will be the form of the work.

(Here a half-page is left blank in the manuscript and is followed by a full blank page.)

The differences used by Newton in his general rule are of the special kind which we have called adjusted differences. Such differences had not, it is believed, been used by any other writer on the subject, until in 1910 Dr. W. F. Sheppard, quite independently, recommended their use in his article on Interpolation in the *Encyclopaedia Britannica*, and gave them the name of adjusted differences, which we have adopted. They have the advantage that the nth adjusted difference obtained from $n+1$ ordinates standing at unequal intervals coincides with the nth ordinary difference obtained from $n+1$ ordinates of the same function standing at equal intervals; and when a, b, c, d are made to differ by unity the general equation obtained from the ordinates U_a, U_b, U_c, U_d, etc., without any readjustment of its elements, assumes at once the familiar form.

A comparison may be made between the divided differences as used in the *Methodus* and in the *Lemma*, and the adjusted differences used in the *Regula*.

The scheme below shows for the two systems the leading differences arising from the ordinates U_a, U_b, U_c, U_d, U_e, U_f, erected at the points a, b, c, d, e, f.

Order of Differences.	Divided Differences of the *Methodus* and *Lemma*.		Adjusted Differences of the *Regula*.	
	Suggested Notation.	Value of Differences.	Suggested Notation.	Value of Differences.
1st	$\grave{\Delta}(ab)$	$\frac{Ua - Ub}{(a-b)}$	$\Delta(ab)$	$\frac{Ua - Ub}{(a-b)}$
2nd	$\grave{\Delta}^2(abc)$	$\frac{\grave{\Delta}(ab) - \grave{\Delta}(bc)}{(a-c)}$	$\Delta^2(abc)$	$\frac{\Delta(ab) - \Delta(bc)}{\frac{1}{2}(a-c)}$
3rd	$\grave{\Delta}^3(abcd)$	$\frac{\grave{\Delta}^2(abc) - \grave{\Delta}^2(bcd)}{(a-d)}$	$\Delta^3(abcd)$	$\frac{\Delta^2(abc) - \Delta^2(bcd)}{\frac{1}{3}(a-d)}$
4th	$\grave{\Delta}^4(abcde)$	$\frac{\grave{\Delta}^3(abcd) - \grave{\Delta}^3(bcde)}{(a-e)}$	$\Delta^4(abcde)$	$\frac{\Delta^3(abcd) - \Delta^3(bcde)}{\frac{1}{4}(a-e)}$
5th	$\grave{\Delta}^5(abcdef)$	$\frac{\grave{\Delta}^4(abcde) - \grave{\Delta}^4(bcdef)}{(a-f)}$	$\Delta^5(abcdef)$	$\frac{\Delta^4(abcde) - \Delta^4(bcdef)}{\frac{1}{5}(a-f)}$

Taking any adjusted difference, for example the fourth, we can express its relationship to the differences of the preceding order in the form

$$\Delta^4(abcde) = \frac{\Delta^3(abcd) - \Delta^3(bcde)}{\frac{1}{4}(a-e)},$$

or in the form $\Delta^3(abcd) = \Delta^3(bcde) + \frac{1}{4}(a-e)\Delta^4(abcde)$.

The latter form will be found to be useful below in arriving at the interpolation formula.

Expressing Newton's process in the above scheme of notation he obtains U_x by working backwards from the sixth difference namely $\Delta^6(abcdefg)$. This is assumed to be the same as $\Delta^6(abcdefx)$, where x replaces g. Then from the law of formation of the differences it follows at once that:

$$\begin{aligned}
\Delta^5(abcdex) &= \Delta^5(abcdef) + \frac{(x-f)}{6}\,\Delta^6(abcdefz),\\
\Delta^4(abcdx) &= \Delta^4(abcde) + \frac{x-e}{5}\,\Delta^5(abcdex),\\
\Delta^3(abcx) &= \Delta^3(abcd) + \frac{x-d}{4}\,\Delta^4(abcdx),\\
\Delta^2(abx) &= \Delta^2(abc) + \frac{x-c}{3}\,\Delta^3(abcx),\\
\Delta\,(ax) &= \Delta\,(ab) + \frac{x-b}{2}\,\Delta^2(abx),\\
U_x &= U_a + (x-a)\Delta(ax).
\end{aligned}$$

A NEW THEOREM.

Newton makes the interesting remark that the rule requires no further demonstration than that the method of passing from the differences to the required term is the same as that by which we pass from the required term to the differences.

This is literally the case. U_x is found simply by reversing the process of finding the final difference. It is the same idea that is used when, being given the cubes of four successive integers, we difference them and then, working backwards, complete the whole scheme of the cubes of the natural numbers.

In the scheme of adjusted differences U_x is incorporated into the scheme of values and is as much a part of the scheme as U_a or U_b. The same is true of a scheme of divided differences. The new point that is included in the scheme of adjusted differences is that it allows U_x for a fractional value of x to be incorporated into a scheme of ordinary differences. Ordinary differences are in fact " adjusted differences " in the case when the values are taken at unit intervals and they fit naturally into a scheme of adjusted differences, though they do not fit into a scheme of divided differences.

This simple unifying proposition fills the gap which many students must have felt in dealing with ordinary differences from the fact that the value to be interpolated was not regarded as essentially a part of the scheme but appeared to be a sort of excrescence upon it. The theorem is a new one, and must find an important place in any complete presentation of the subject.

AN EXAMINATION OF THE SCHEME.

In saying that he preferred to express *rem omnem* in a single general rule, there can be little doubt that he meant to be understood literally, and that every proposition or formula employed in the subject under discussion is intended to be derived from the scheme, not by a process of algebraic deduction as in the *Methodus*, but at sight.

Newton has not given detailed explanations how this should be done, but the idea having been suggested it will be interesting to examine the scheme from this point of view.

Let us say here that the student who is dependent on algebraical proofs misses the beautiful simplicity of the subject. Its various propositions should be visible in their own light.

We proceed to examine Newton's scheme under different headings.

FORMULAS IN TERMS OF DIFFERENCES.

The interpolation formula derived from adjusted differences comes out in the form :

$$\begin{aligned} U_x = U_a &+ (x-a)\Delta(ab) \\ &+ \frac{(x-a)(x-b)}{1\,.\,2}\Delta^2(abc) \\ &+ \frac{(x-a)(x-b)(x-c)}{1\,.\,2\,.\,3}\Delta^3(abcd) \\ &+ \frac{(x-a)(x-b)(x-c)(x-d)}{1\,.\,2\,.\,3\,.\,4}\Delta^4(abcde), \text{ etc.} \end{aligned}$$

If we write $a=0$, $b=1$, $c=2$, $d=3$, etc., the equation becomes

$$U_x = U_0 + x\Delta U_0 + \frac{x(x-1)}{1\,.\,2}\Delta^2 U_0 + \frac{x(x-1)(x-2)}{1\,.\,2\,.\,3}\Delta^3 U_0, \text{ etc.}$$

Writing A for $x-a$, B for $x-b$, C for $x-c$,

and so on, we can express the general equation in a more condensed form without losing sight of the composition of its terms, as follows :

$$\begin{aligned} U_x = U_a + A\Delta(ab) + \frac{AB}{1\,.\,2}\Delta^2(abc) + \frac{ABC}{1\,.\,2\,.\,3}\Delta^3(abcd) \\ + \frac{ABCD}{1\,.\,2\,.\,3\,.\,4}\Delta^4(abcde), \text{ etc.} \end{aligned}$$

There are several observations to be made about this equation.

(1) The quantities a, b, c, d, ... are not subject to any condition as to the order in which they shall be taken. In fact, a given set of terms a, b, c, d, e ... can be taken in any order provided they are all included. For every different permutation of the quantities there is a distinct set of differences. But if the differences are carried to the final difference, the result is always the same.

In practice, of course, it is usually convenient to take the quantities a, b, c, d, ... in the order of their magnitude, and we have therefore to consider one set of differences only.

The fact, however, that the quantities can be placed in any order has the great convenience that we can place the unknown value where it is least in the way, at the beginning or at the end of the scheme.

(2) Having settled the order of the quantities a, b, c, d, e, ... we can start the general equation with any of the corresponding values of U. We have made U_a the first term of the general equation as written above, but since,

$$U_a = U_b + (a-b)\Delta(ab),$$
$$U_b = U_c + (b-c)\Delta(bc).$$

it is quite plain, that we can make U_b, or U_c, or any other values the first term.

(3) The value of the difference used in any term determines the coefficient of the next term.

Thus U_a is followed by the coefficient A, that is by $(x-a)$,
$\Delta\,(ab)$,, ,, ,, ,, AB ,, $(x-a)(x-b)$,
$\Delta^2(abc)$,, ,, ,, ,, ABC ,, $(x-a)(x-b)(x-c)$,

the sequences are invariable, and the law is obvious.

(4) The quantities entering into the difference that appears in any term are all found in the difference of the next term with one more added.

Thus $\Delta\,(ab)$ is followed by $\Delta^2(abc)$,
$\Delta^2(abc)$,, ,, $\Delta^3(abcd)$.

Now from the law of formation of the differences there are never more than two differences that satisfy this condition. Thus if β be the quantity next before a, $\Delta^2(abc)$ can be followed by $\Delta^3(\beta abc)$ or by $\Delta^3(abcd)$. The new quantity in the next term must be next before the first, or next after the last of the quantities already used.

Supposing the quantities to be arranged in vertical order, we can go up one step, or down one step.

Following these rules we can write down equations automatically. For example, from the values at the points, a, b, c, d, ... we can write:

$$U_x = U_b + B\Delta(bc) + \frac{BC}{1\,.\,2}\,\Delta^2(bcd) + \frac{ABC}{1\,.\,2\,.\,3}\,\Delta^3(abcd),$$

or
$$= U_b + B\Delta(ab) + \frac{AB}{1\,.\,2}\,\Delta^2(abc) + \frac{ABC}{1\,.\,2\,.\,3}\,\Delta^3(abcd),$$

or
$$= U_c + C\Delta(cd) + \frac{CD}{1\,.\,2}\,\Delta^2(bcd) + \frac{BCD}{1\,.\,2\,.\,3}\,\Delta^3(abcd), \text{ etc.,}$$

and all these equations give the same result.

Thus we can write down the central difference formulas without any special investigation.

Suppose a to be the central quantity and be preceded by b', c', d', and followed by b, c, d, the terms next to a being b and b'.

The following scheme of the central differences will help us to determine the differences that are to appear in the successive terms :

$$\begin{array}{lllll}
c' & & & & \\
b' & & & & \\
 & \Delta(b'a) & & \Delta^3(c'b'ab) & \\
a & & \Delta^2(b'ab) & & \Delta^4(c'b'abc) \\
 & \Delta(ab) & & \Delta^3(b'abc) & \\
b & & & & \\
c & & & &
\end{array}$$

Starting with U_a follow the central line of differences, taking the upper odd differences:

$$\begin{aligned} U_x = U_a + A\Delta(b'a) + \frac{B'A}{1.2}\Delta^2(b'ab) & \\ + \frac{B'AB}{1.2.3}\Delta^3(c'b'ab) & \\ + \frac{C'B'AB}{1.2.3.4}\Delta^4(c'b'abc). & \end{aligned}$$

Then take the lower odd differences :

$$\begin{aligned} U_x = U_a + A\Delta(ab) + \frac{AB}{1.2}\Delta^2(b'ab) & \\ + \frac{B'AB}{1.2.3}\Delta^3(b'abc) & \\ + \frac{B'ABC}{1.2.3.4}\Delta^4(c'b'abc). & \end{aligned}$$

Now take the mean of the two formulas for U_x.

$$\begin{aligned} U_x = U_a + A\,\tfrac{1}{2}(\Delta(b'a) + \Delta(ab)) + \tfrac{1}{2}\left(\frac{B'A}{1.2} + \frac{AB}{1.2}\right)\Delta^2(b'ab) & \\ + \frac{B'AB}{1.2.3}\tfrac{1}{2}(\Delta^3(c'b'ab) + \Delta^3(b'abc)) & \\ + \tfrac{1}{2}\left(\frac{C'B'AB}{1.2.3.4} + \frac{B'ABC}{1.2.3.4}\right)\Delta^4(c'b'abc). & \end{aligned}$$

Make $c'b'abc = -2, -1, 0, 1, 2.$

Then

$$\begin{aligned} A &= x, \\ B'A + AB &= (x+1)x + x(x-1) = 2x^2, \\ B'AB &= (x+1)x(x-1) = x(x^2-1), \\ C'B'AB + B'ABC &= (x+2)(x+1)x(x-1) + (x+1)x(x-1)(x-2) \\ &= 2x^2(x^2-1). \end{aligned}$$

We may use $\bar{\Delta}U_0$, $\bar{\Delta}^2U_0$, etc., to denote the central differences or mean differences in line with U_0. Thus the result is :

$$U_x = U_0 + x\bar{\Delta}U_0 + \frac{x^2}{2}\bar{\Delta}^2U_0 + \frac{x(x^2-1)}{6}\bar{\Delta}^3U_0 + \frac{x^2(x^2-1)}{24}\bar{\Delta}^4U_0 + \text{etc.}$$

This is Stirling's formula.

We have described the process at length, but in practice the formula can be written down at sight.

Similarly, taking two central terms a and α' we can obtain Bessel's formula.

It will be noticed that the formulas for unequal intervals present no more difficulty than those for equal intervals.

The laws of formation of the formulas were given by Dr. W. F. Sheppard in his article on Finite Differences in the *Encyclopaedia Britannica*, Eleventh Edition, 1910, as follows :

(1) We can start with any value.
(2) We can pass to successive differences by steps up and down.
(3) The new suffix determines the new value.

FORMULAS IN TERMS OF VALUES OF THE FUNCTION.

Since the central difference formulas can, as we have shown above, be derived by a simple automatic process from the single general rule, Newton's claim that it includes *rem omnem* is so far justified.

But in the *Regula* he gives expressions for the central ordinate in terms of the values of the given ordinates, with an estimate of the error in each case. It is necessary to show how these may be obtained from the single general rule. The problem can be reduced to that of finding an expression for the nth difference in terms if the n given values and of the required value U_x.

Taking the scheme of divided differences U_a is involved in the successive differences $\grave{\Delta}(ab)$, $\grave{\Delta}^2(abc)$, $\grave{\Delta}^3(abcd), \ldots$, $\grave{\Delta}^n(abc \ldots x)$, and if the method of formation of the differences be examined it is evident that the term involving U_a in the final difference $\grave{\Delta}^n(abc \ldots x)$ is

$$\frac{U_a}{(a-b)(a-c)(a-d)\ldots(a-x)}.$$

The terms involving U_b, U_c, U_d, etc., are similar in form, and the final difference is

$$\begin{aligned}\grave{\Delta}^n(abc \ldots x) &= \frac{U_a}{(a-b)(a-c)\ldots(a-x)} \\ &+ \frac{U_b}{(b-a)(b-c)\ldots(b-x)} \\ &+ \frac{U_c}{(c-a)(c-b)\ldots(c-x)} \\ &+ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ &+ \frac{U_x}{(x-a)(x-b)(x-c)\ldots}.\end{aligned}$$

For example
$$\frac{U_a}{a-b} + \frac{U_b}{b-a} = \grave{\Delta}(ab),$$

$$\frac{U_a}{(a-b)(a-c)} + \frac{U_b}{(b-a)(b-c)} + \frac{U_c}{(c-a)(c-b)} = \grave{\Delta}^2(abc) \text{ etc.}$$

The general expression for the nth divided difference, as above, was given by Euler, and enables us at once to express U_x in terms of U_a, U_b, $U_c, \ldots$, the error in the value so obtained being $(x-a)(x-b)(x-c)\ldots \times \grave{\Delta}^n(abc \ldots x)$,

or
$$\frac{(x-a)(x-b)(x-c)\ldots}{\underline{|n}} \times \Delta^n(abc \ldots x).$$

At a later date than Euler, Lagrange gave as a formula for U_x :

$$\begin{aligned}U_x &= \frac{(x-b)(x-c)(x-d)\ldots}{(a-b)(a-c)(a-d)\ldots} \times U_a \\ &+ \frac{(x-a)(x-c)(x-d)\ldots}{(b-a)(b-c)(b-d)\ldots} \times U_b \\ &+ \frac{(x-a)(x-b)(x-d)\ldots}{(c-a)(c-b)(c-d)\ldots} \times U_c \\ &+ \text{etc.},\end{aligned}$$

which is merely Euler's formula in another shape.

There is an interesting method of showing in diagrammatic form the interpolation formulas that can be derived from the single general rule. What follows is adapted and extended from a paper which was published in the *Journal of the Institute of Actuaries* for April, 1909.

We have already pointed out that alternative expressions for U_x can be written down automatically and are identities, as, for example, to second differences :

$$U_a + A\Delta(ab) + \frac{AB}{1.2}\Delta^2(abc),$$

$$U_b + B\Delta(ab) + \frac{AB}{1.2}\Delta^2(abc),$$

$$U_b + B\Delta(bc) + \frac{BC}{1.2}\Delta^2(abc),$$

$$U_c + C\Delta(bc) + \frac{BC}{1.2}\Delta^2(abc),$$

$$U_c + C\Delta(bc) + \frac{BC}{1.2}\Delta^2(bcd),$$

if we write these as follows :

$$\begin{tikzcd}
U_a + A \arrow[dr, dash] & & \\
& \Delta(ab) + \frac{1}{2}AB \arrow[dr, dash] & \\
U_b + B \arrow[ur, dash] \arrow[dr, dash] & & \Delta^2(abc) \\
& \Delta(bc) + \frac{1}{2}BC \arrow[ur, dash] \arrow[dr, dash] & \\
U_c + C \arrow[ur, dash] & & \Delta^2(bcd)
\end{tikzcd}$$

a hexagonal diagram is immediately suggested.

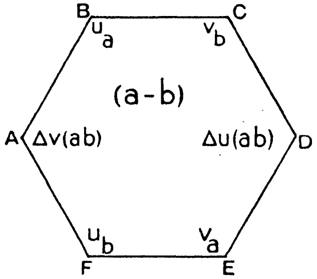


FIG. 4.

In this figure, $$\Delta V(ab) = \frac{V_a - V_b}{a - b},$$

$$\Delta U(ab) = \frac{U_a - U_b}{a - b}.$$

If the inclined lines represent the products of the quantities they connect, so that $AB = U_a \times \Delta V(ab)$; and if we adopt the convention that a line taken rightwards gives the +ve sign to the product, and a line taken leftwards the negative sign, so that $DE = -V_a \times \Delta U(ab)$, then it is easy to show that

$$AB + CD + DE + FA = 0,$$

that is, *the circuit of the cell is zero.*

The semi-circuit $AB + CD$ is = the semi-circuit $AF + ED$, and each of these is equal to the difference of the inclined diagonals, namely $FC - BE$.

It will be noticed that the relative positions of V_a, V_b, and $\Delta V(ab)$ are inverted and reversed as compared with the relative positions of U_a, U_b, and $\Delta U(ab)$.

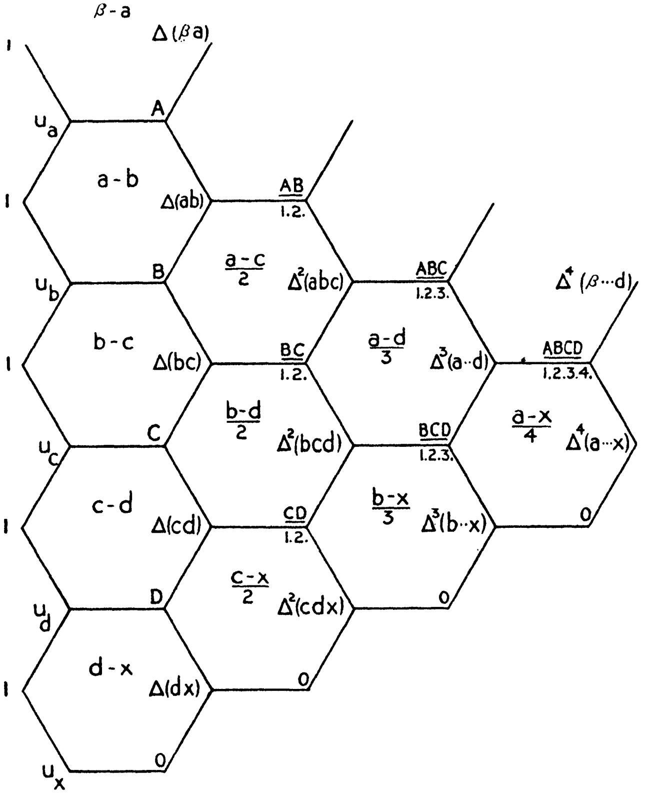


FIG. 5.

In the second diagram a scheme is given which includes the known values U_a, U_b, U_c, U_d, and the unknown value U_x. The known values beyond U_a are understood to be continued by values U_β, U_γ, etc. The letters A, B, C, D in the scheme are written for $x-a$, $x-b$, $x-c$, $x-d$, and the differences are adjusted differences.

Each cell has the same properties as the cell in the first diagram.

Thus the circuit of each cell is zero; and hence the sum of the circuits of all the cells is zero. But the products represented by internal lines all cancel out, each being taken once positively and once negatively. There remain only the external lines, and therefore *the circuit of any group of cells is zero.*

It follows that if in going from left to right we pass from any chosen coefficient, say the coefficient equal to unity between U_b and U_c, to any chosen difference, say $\Delta^3(abcd)$, the result is identically the same whatever route be taken ; thus :

$$U_b + B\Delta(bc) + \frac{BC}{1.2}\Delta^2(abc) + \frac{ABC}{1.2.3}\Delta^3(abcd)$$

$$= U_c + C\Delta(cd) + \frac{CD}{1\cdot 2}\Delta^2(bcd) + \frac{BCD}{1.2.3}\Delta^3(abcd).$$

In the second diagram suppose fourth differences to be constant, so that the fourth adjusted difference $\Delta^4(abcdx)$ is a known value, then every route which extends right across the figure will give the same result.

But if we take the lowest route we simply get the term U_x, since it is followed by a series of terms the coefficients of which are all zero.

If we take the uppermost route of the closed cells we get

$$U_a + A\Delta(ab) + \frac{AB}{1.2}\Delta^2(abc) + \frac{ABC}{1.2.3}\Delta^3(abcd) + \frac{ABCD}{1.2.3.4}\Delta^4(abcdx).$$

This is therefore a formula for U_x.

By choosing routes which follow the central differences we obtain the central difference formulas. It is not necessary to repeat them, but attention may be drawn to an interesting variation of central difference formulas obtained by using the property mentioned in connection with the first diagram that the semi-circuit of any cell is equivalent to the difference of the inclined diagonals.

Starting with the cell which contains the terms Ub and Uc, the route across the diagram can be written :

$$B\,.\,U_c + \frac{ABC}{1.2.3}\Delta^2(bcd) + \text{etc.},$$

$$-C\,.\,U_b - \frac{BCD}{1.2.3}\Delta^2(abc) - \text{etc.},$$

a formula which involves only even differences.

In the same way, starting with the term U_c, we can write :

$$U_c + \frac{B.C}{1.2}\,.\,\Delta(cd) + \frac{ABCD}{1.2.3.4}\Delta^3(abcd)$$

$$-\frac{C.D}{1.2}\Delta(bc) - \text{etc.},$$

a formula which involves only odd differences.

With a diagram as above for any particular case it is easy to pick out the most appropriate formula which will usually be the one that involves the smallest coefficients.

A similar diagram can be used for divided differences and for ordinary differences. Ordinary differences are, in fact, adjusted differences for the case when the given values correspond to successive integral values of x. By acting on the theory of adjusted differences U_x for a fractional value of x can be incorporated into the same scheme as the ordinary differences. This is an entirely new idea and has been the one point in which the theory of the subject has been imperfect.

The third figure is a diagram for the ordinary differences, U_x being incorporated into the scheme on the principle of adjusted differences.

In the diagram Δ_{-2} is written for ΔU_{-2} and x_2 means $\frac{x(x-1)}{1.2}$, etc.

Without further explanation we can read formulas of the scheme as follows:
Using descending differences:

$$U_x = U_0 + x\Delta U_0 + x_2\Delta^2 U_0 + x_3\Delta^3 U_0 + \text{etc.} \ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{I.}$$

Using ascending differences:

$$U_x = U_0 + x\Delta U_{-1} + (x+1)_2\Delta^2 U_{-2} + (x+2)_3\Delta^3 U_{-3} + \text{etc.} \ldots\ldots\ldots \text{II.}$$

Gauss's Formula, taking odd differences above the central line,

$$U_x = U_0 + x\Delta U_{-1} + (x+1)_2\Delta^2 U_{-1} + (x+1)_3\Delta^3 U_{-2} + \text{etc.} \ldots\ldots\ldots \text{III.}$$

Gauss's Formula, taking odd differences below the central line,

$$U_x = U_0 + x\Delta U_0 + x_2\Delta^2 U_{-1} + (x+1)_3\Delta^3 U_{-1} + \text{etc.} \ldots\ldots\ldots\ldots\ldots \text{IV.}$$

Stirling's Formula; this is the mean of III and IV:

$$U_x = U_0 + x\Delta\frac{U_0 + U_{-1}}{2} + \frac{x^2}{2}\Delta^2 U_{-1} + \frac{x(x^2-1)}{6}\Delta^3\frac{U_{-2}+U_{-1}}{2} + \text{etc.} \ldots\ldots \text{V.}$$

Gauss's Formula, making u_1 the initial term and using upper odd differences:

$$U_x = U_1 + (x-1)\Delta U_0 + x_2\Delta^2 U_0 + x_3\Delta^3 U_{-1} \ldots + \text{etc.} \ldots\ldots\ldots\ldots\ldots \text{VI.}$$

Bessel's Formula; this is the mean between IV and VI:

$$U_x = \frac{U_0 + U_1}{2} + (x - \tfrac{1}{2})\Delta U_0 + x_2\Delta^2\frac{U_0 + U_{-1}}{2} + \frac{(x-\frac{1}{2})x(x-1)}{6}\Delta^3 U_{-1} + \text{etc.} \ldots \text{VII.}$$

or changing x into $x + \frac{1}{2}$,

$$U_{x+\frac{1}{2}} = \frac{U_0 + U_1}{2} + x\Delta U_0 + \frac{x^2 - \frac{1}{4}}{1\,.\,2}\Delta^2\frac{U_0 + U_1}{2} + \frac{x(x^2 - \frac{1}{4})}{6}\Delta^3 U_{-1} + \text{etc.} \ldots\ldots \text{VIII.}$$

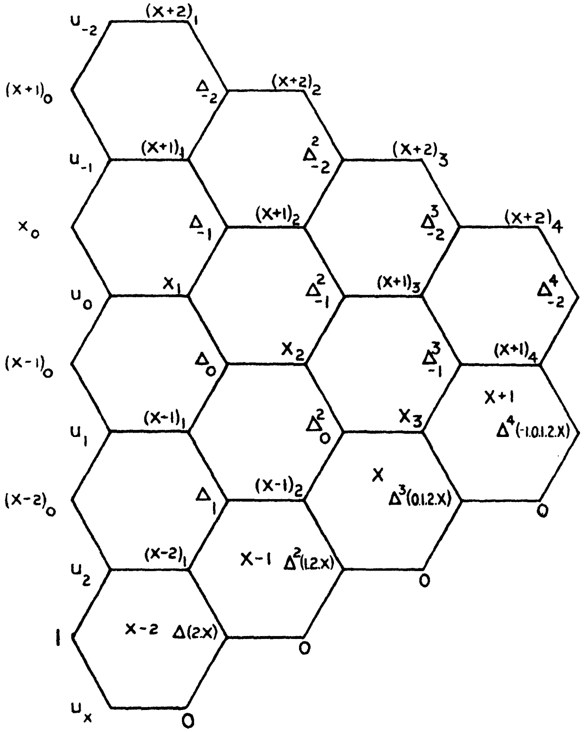


FIG. 6.

Everett's Formula. In obtaining this formula we use the property that the difference of the inclined diagonals is the same as the semi-circuit of the cell.

$$\begin{aligned} U_x = {} & xU_1 + (x+1)_3\Delta^2 U_0 + (x+2)_5\Delta^4 U_{-1} + \text{etc.} \\ & -(x-1)U_0 - x_3\Delta^2 U_{-1} - (x+1)_5\Delta^4 U_{-2} - \text{etc.} \end{aligned}$$

Putting $x=p$, $1-x=q$, this becomes

$$\begin{aligned} U_p = {} & pU_1 + \frac{p(p^2-1)}{6}\Delta^2 U_0 + \frac{p(p^2-1)(p^2-4)}{120}\Delta^4 U_{-1} + \text{etc.} \\ & + qU_0 + \frac{q(q^2-1)}{6}\Delta^2 U_{-1} + \frac{q(q^2-1)(q^2-4)}{120}\Delta^4 U_{-2} + \text{etc.} \quad \ldots\ldots\ldots \text{IX.} \end{aligned}$$

Another form of Everett's Formula can be given in which only odd differences appear. Starting with the term U_0, we can write:

$$\begin{aligned} U_x = {} & U_0 + (x+1)_2\Delta U_0 + (x+2)_4\Delta^3 U_{-1} + (x+3)_6\Delta^5 U_{-2} + \text{etc.} \\ & - x_2\Delta U_{-1} - (x+1)_4\Delta^3 U_{-2} - (x+2)_6\Delta^5 U_{-3} - \text{etc.}, \end{aligned}$$

or putting $x=p-\frac{1}{2}$, $1-x=q+\frac{1}{2}$,

$$\begin{aligned} U_{p-\frac{1}{2}} = {} & U_0 + \frac{p^2-\frac{1}{4}}{1\,.\,2}\Delta U_0 + \frac{(p^2-\frac{1}{4})(p^2-\frac{9}{4})}{1\,.\,2\,.\,3\,.\,4}\Delta^3 U_{-1} + \text{etc.} \\ & - \frac{q^2-\frac{1}{4}}{1\,.\,2}\Delta U_{-1} - \frac{(q^2-\frac{1}{4})(q^2-\frac{9}{4})}{1\,.\,2\,.\,3\,.\,4}\Delta^3 U_{-2} + \text{etc.} \quad \ldots\ldots\ldots\ldots \text{X.} \end{aligned}$$

Mr. G. J. Lidstone has pointed out in the *Proceedings of the Edinburgh Mathematical Society*, Vol. XL., that Everett's Formula had been previously given by Laplace. This is an example of what has continually happened in this subject, of formulas being given and forgotten and rediscovered. Professor Everett himself made an astonishing communication to the British Association in the year 1900, in which he said, " I have also found in Lemma 5, which follows Proposition 40 of the third book of the *Principia*, what is doubtless the earliest statement of the ordinary interpolation formula. . . . The formula appears hitherto to have escaped recognition."

Stirling gave not only the formula which bears his name, but a formula equivalent to what we call Bessel's in the work for which he borrowed the title of Newton's Tract *Methodus Differentialis* (1730). There was no want of acknowledgement on his part of the priority of Newton, of whom he says :

" Many celebrated mathematicians after Newton have discussed the question of describing a curve of the parabolic order through given points. But all their solutions are the same as those given above, which hardly differ from those of Newton in the 5th Lemma of the third book of the *Principia*, and in the *Methodus Differentialis* edited by Mr. Jones. Newton describes a parabolic curve through given points ; others have considered the determination of the curve from definite data ; but however it may be expressed and however it may be carried out, it is the same problem, and certainly the discovery of the mathematical forms for the values of the interpolated ordinate is exceedingly ingenious and worthy of the celebrity of its author ; but after the mathematical forms are given the investigation of the problem is easy, nothing more being required than the solution of simple equations."

NEWTON'S WORK IN OPTICS.

BY PROF. E. T. WHITTAKER, SC.D., F.R.S.

IN February 1692 Newton left a light burning in his rooms when he went to chapel, "which, by unknown means, destroyed his papers, and among them a large work on Optics, containing the experiments and researches of twenty years."[*] The damage was partially repaired by 1704, when the extant treatise on *Opticks* was published. "Part of the ensuing discourse about Light," he says, "was written at the desire of some Gentlemen of the Royal Society, in the year 1675, and then sent to the Secretary, and read at their meetings, and the rest was added about twelve years after to complete the theory: except the Third Book, and the last Proposition of the Second, which were since put together out of scattered Papers." Other editions, containing additional "Queries," appeared in 1717, 1721, and 1730; the last, "corrected by the author's own hand, and left before his death with his bookseller." The *Lectiones Opticae*, which he gave at Cambridge in 1669, were published first in an English translation in 1728. They had, however, been "deposited, at the time they were read, amongst the archives of the University: from whence many copies have been taken, and handed about by the curious in these matters."

From these, with the papers in the *Philosophical Transactions* and other sources referred to below, we have ample materials for an account of Newton's work in Optics.

In the last last of the seventeenth century, when his discoveries were made, the theories of Descartes were dominant. According to the Cartesian philosophy, all space—even at the remotest distances beyond the stars—is a plenum, so that a particle can move only by taking the place of other particles which themselves are displaced. Light was imagined to be essentially a pressure[†] transmitted through this dense mass of particles: vision might therefore be compared to the perception of the presence of objects which a blind man obtains by the use of his stick, the transmission of pressure along the stick from the object to the hand being analogous to the transmission of pressure through the plenum of space from a luminous object to the eye.

Descartes supposed "the diversities of colour and light" to be due to the different ways in which the matter moves:[‡] the various colours were connected with different rotatory velocities of the particles, those which rotate most rapidly giving the sensation of red, the slower ones of yellow, and the slowest of green and blue.

About the time that Newton began his researches, the Cartesian doctrine was challenged by Robert Hooke, who advanced the view[§] that light is a small and rapid vibratory motion of the medium, and that colour depends on the form of the light-pulse: "blue is an impression on the retina of an oblique and confused pulse of light, whose weakest part precedes and whose strongest follows" : while in red, the strongest part precedes and the weakest follows.[*]

---

[*] De la Pryme's diary; cf. Weld, i. p. 371. This accident was, no doubt, the origin of the story of the dog Diamond.

[†] Descartes, *Principia*, Part iii. §§ 55-64; *Dioptique*, Discours premier.

[‡] Descartes, *Principia*, Part iv. § 195; *Météores*, Discours huitième.

[§] Hooke's *Micrographia* (1667).

Of definite properties of light, the predecessors of Newton were acquainted with the laws of reflection and refraction, with diffraction (discovered in the middle of the seventeenth century by the Jesuit Grimaldi), and with the iridescent colours of thin plates.

In 1666 Newton, then aged twenty-three and interested in the construction and performance of telescopes, felt the desirability of reducing or removing the chromatic aberration of lenses, and so was led to examine the formation of a coloured spectrum by a prism. For this purpose, "having darkened my chamber, and made a small hole in my window-shuts, to let in a convenient quantity of the Sun's light, I placed my Prisme at his entrance, that it might be thereby refracted to the opposite wall." Observing that the length of the coloured spectrum was many times greater than its breadth, he was led after more experiments to the view that ordinary white light is really a mixture of rays of every variety of colour, and that the elongation of the spectrum is due to the differences in the refractive power of the glass for these different rays. "To the same degree of Refrangibility ever belongs the same colour, and to the same colour ever belongs the same degree of Refrangibility."

This discovery, made before he was driven from Cambridge by the plague in 1666,[†] was published in the Royal Society's *Transactions* in 1672. Its appearance gave rise to an acute controversy.[‡] Hooke in particular assailed it vehemently ; and the unpleasant consequences which followed its announcement had much to do with the reluctance which Newton ever afterwards showed to make known his results to the world.[§] At the time indeed he seriously contemplated abandoning research altogether. "I intend," he wrote to Oldenburg, "to be no farther solicitous about matters of Philosophy : and therefore I hope you will not take it ill, if you find me never doing anything more in that kind."

In the course of the discussion Newton took occasion to explain more fully his views on the nature of light. Hooke charged him with holding the doctrine that light is a material substance. Now Newton had, as a matter of fact, a great dislike of the more speculative kind of hypotheses ; his aim was to create a theory based directly on observation and free from all imaginings as to the hidden mechanism of things. "He used," says Maclaurin,[||] "to call his philosophy *experimental philosophy*, intimating, by the name, the essential difference there is betwixt it and those systems that are the product of genius and invention only." Accordingly, in reply to Hooke's criticism, he protested [¶] that his views on colour were in no way bound up with any particular conception of the ultimate nature of optical processes.

He was, however, unable to carry out his plan of connecting together the phenomena of light into a coherent and reasoned whole without having recourse to hypotheses. The conjecture of Hooke, that light consists in vibrations of an aether, he rejected for reasons which at the time were perfectly cogent, and which indeed were not successfully refuted for over a century. One of these was the incompetence of the wave-theory to account for the rectilinear propagation of light, and another was its inability to embrace the facts—announced by Huygens in 1690 and first interpreted correctly by Newton himself—of polarisation. On the whole, he seems to have inclined to a scheme of which the following may be taken as a summary: *

---

[*] *Micrographia*. p. 64.

[†] *Phil. Trans*. No. 80, Feb. 19, 167$\frac{1}{2}$. This was Newton's first scientific paper.

[‡] Cf. *e.g.* the objections of the astronomer Flamsteed in his letter to Collins of April 17, 1672, printed in Rigaud, ii. p. 134.

[§] "The warm opposition his admirable discoveries in optics met with, in his youth, deprived the world of a full account of them for many years, till there appeared a greater disposition amongst the learned to receive them, and induced him to retain other important inventions by him, from an apprehension of the disputes in which a publication might involve him." Maclaurin's *Account of Sir Isaac Newton's Phil. Disc.* (1748), p. 13.

[||] *Account*, p. 8.

[¶] *Phil. Trans.* 7 (1672), p. 5086.

All space is permeated by an elastic medium or *aether*, which pervades the pores of all material bodies, and is the cause of their cohesion: its density varies from one body to another, being greatest in the free interplanetary spaces. It is not necessarily a single uniform substance; but just as air contains aqueous vapour, so the aether may contain various "aethereal spirits," adapted to produce the phenomena of electricity, magnetism, and gravitation.

The vibrations of the aether cannot, for the reasons already mentioned, be supposed in themselves to constitute light. Light is therefore taken to be "something of a different kind, propagated from lucid bodies." Those who wish "may suppose light any corporeal emanation, or any impulse or motion of any other medium or aethereal spirit diffused through the main body of aether, or what else they can imagine proper for this purpose. To avoid dispute, and make this hypothesis general, let every man here take his fancy: only whatever light be, I suppose it consists of rays differing from one another in contingent circumstances, as bigness, form, or vigour." †

In any case, light and aether are capable of mutual interaction; aether is, in fact, the intermediary between light and ponderable matter; for instance, in refraction, and when a ray of light meets a stratum of aether denser or rarer than that through which it has lately been passing, it is, in general, deflected from its rectilinear course on account of the differences of density of the aether between one material medium and another. The condensation or rarefaction of the aether due to a material body extends to some little distance from the surface of the body, so that the inflexion due to it is really continuous, and not abrupt: and this further explains diffraction, which Newton took to be "only a new kind of refraction, caused, perhaps, by the external aether's beginning to grow rarer a little before it came at the opake body, than it was in free spaces."

Assuming with Newton that light is not actually constituted by the vibrations of an aether, even though such vibrations may exist in close connexion with it, the most definite and easily conceived supposition is that rays of light are streams of corpuscles emitted by luminous bodies. Although this was not the hypothesis of Descartes himself, it was so much akin to his general scheme that the scientific men of Newton's generation, who were for the most part deeply imbued with the Cartesian philosophy, instinctively selected it from the wide choice of hypotheses which Newton had offered them: and by later writers it was generally associated with Newton's name.‡

The discovery that ordinary white light is a heterogeneous mixture of differently refrangible rays led Newton to realise more fully the difficulty of getting rid of chromatic aberration in lenses, and disposed him to turn to reflection rather than refraction as the basic principle of optical instruments.

---

* Cf. his memoir in *Phil. Trans.* 7 (1672): his memoir presented to the Royal Society in Dec. 1675, which is printed in Birch, iii. p. 247; the later editions of his *Opticks*, especially Queries 18, 19 20, 21, 23, 29; the Scholium at the end of the *Principia*; and a letter to Boyle, of date Feb. 167$\frac{8}{9}$, printed in Horsley, iv. p. 385, and in Rigaud, ii. p. 407.

In the *Principia*, Book i. § xiv., the analogy between rays of light and streams of corpuscles is indicated, but Newton does not commit himself to any theory of light based on this.

† Royal Society, Dec. 9, 1675.

‡ The ideas current in 1713, nine years after the publication of the *Opticks*, may be illustrated from Berkeley's first dialogue between Hylas and Philonous:

Phil. How! is light then a substance?

Hyl. I tell you, Philonous, external light is nothing but a thin fluid substance, whose minute particles being agitated with a brisk motion, and in various manners reflected from the different surfaces of outward objects to the eyes, communicate different motions to the optic nerves.

In 1668 he invented the type of reflecting telescope which bears his name,* and of which an example was sent to the Royal Society in 1671. In the description, *An Accompt of a New Catadioptrical Telescope*,† it is stated that "the objects are magnified about 38 times," whereas "an ordinary telescope of about two feet long only magnifies 13 or 14 times."

After this he returned to the consideration of colour. Having made sure that colour is an inherent characteristic of light, he inferred that it must be associated with some definite quality of the corpuscles or aether-vibrations. The corpuscles corresponding to different colours would, he remarked, like sonorous bodies of different pitch, excite vibrations of different types in the aether: and "if by any means those [aether-vibrations] of unequal bignesses be separated from one another, the largest beget a sensation of a *Red* colour, the least or shortest of a deep *Violet*, and the intermediate ones, of intermediate colours." ‡

This sentence is the first enunciation of the principle that homogeneous light is essentially *periodic* in its nature, and that differences of period correspond to differences of colour. The analogy with Sound is obvious: "much after the manner that the Vibrations of the Air, according to their several bignesses, excite sensations of several Sounds," he says: § and it may be remarked in passing that Newton's theory of periodic vibrations in an elastic medium, which he developed || in connexion with the explanation of Sound, would alone entitle him to a place among those who have exercised the greatest influence on the theory of light, even if he had made no direct contribution to the latter subject.

He devoted considerable attention to the colours of thin plates, and determined the empirical laws of the phenomenon with great accuracy. In order to explain them, he supposed that "every Ray of Light in its passage through any refracting Surface is put into a certain transient Constitution or State, which in the progress of the Ray returns at equal Intervals, and disposes the Ray at every return to be easily transmitted through the next refracting Surface, and between the returns to be easily reflected by it." ¶ The interval between two consecutive dispositions to easy transmission, or "length to fit," he supposed to depend on the colour, being greatest for red light and least for violet. If then a ray of homogeneous light falls on a thin plate, its fortunes as regards transmission and reflexion at the two surfaces will depend on the relation which the length of fit bears to the thickness of the plate: and on this basis he built up a theory of the colours of thin plates. It is evident that Newton's "length of fit" corresponds in some measure to the quantity which in the undulatory theory is called the wave-length of the light: but, as he says, "What kind of action or disposition this is, whether it consists in a circulating or a vibrating motion of the Ray, or of the Medium, or something else, I do not here enquire." **

One more discovery remains to be mentioned. Huygens in his *Traité de la lumière* (1690) had described certain experimental discoveries connected with the rays obtained by double refraction in "Island Crystal." Newton in 1717 showed †† that Huygens' observations made it necessary to suppose that a ray obtained by double refraction differs from a ray of ordinary light in the same way as a long rod whose cross-section is a rectangle differs from a long rod whose cross-section is a circle: in other words, the properties of

---

* In which a small plane mirror inclined at 45° to the axis of the instrument intercepts the rays reflected by the large mirror a little before they arrive at the focus, and so throws them to the side of the tube, where they enter the eyepiece.

† *Phil. Trans.* No. 81.

‡ *Phil. Trans.* 7 (1672), p. 5088.

§ *Opticks*, Query 13.

|| *Principia*, Book ii, Props. xliii.-l.

¶ *Opticks*, Book ii. Prop. xii.

** *Opticks*, ed. 1730, p. 255.

†† *Opticks*, ed. 1717, Query 26.

a ray of ordinary light are the same with respect to all directions at right angles to its direction of propagation, whereas a ray obtained by double refraction has properties related to special directions at right angles to its own direction. "Every Ray of Light," said Newton, "has therefore two opposite Sides, originally endowed with a Property on which the unusual Refraction depends, and the other two opposite sides not endued with that Property." The refraction of such a ray at the surface of a crystal depends on the relation of its sides to the principal plane of the crystal.

That a ray of light should possess such properties seemed to him an insuperable objection to the hypothesis that light is constituted of waves analogous to waves of sound. "For Pressions or Motions," he says,* "propagated from a shining Body through an uniform Medium, must be on all sides alike: whereas by those Experiments it appears, that the Rays of Light have different properties in their different Sides. . . . To me, at least, this seems inexplicable, if Light be nothing else than Pression or Motion propagated through Aether." And on this point he was perfectly right. The concept of the elastic-solid aether with transverse vibrations, by which the paradox was solved, was as yet in the remote future. E. T. WHITTAKER.

---

* *Opticks*, Query 28.

THEED'S STATUE AT GRANTHAM.

NEWTON'S PROBLEM OF THE SOLID OF LEAST RESISTANCE.

BY PROFESSOR A. R. FORSYTH, SC.D., LL.D., F.R.S.

THE PROBLEM.

1. In his *Philosophiæ Naturalis Principia Mathematica*, first published in 1687, Newton propounded his problem[*] of the solid of revolution which experiences the least resistance in moving through a medium in the direction of its axis. After stating, in geometrical terms, the property of the generating curve defined by the first integral of the characteristic equation, he concludes with the words :

> Solidum, quod figuræ hujus revolutione circa axem facta describitur, ... minus resistetur quam aliud quodvis eadem longitudine et latitudine descriptum Solidum circulare.

The result is stated without any indication of the argument that leads to the conclusion.

2. In a famous memoir,[†] containing his first statement of a new criterion leading to an analytical discrimination between maxima and minima, Legendre discussed the Newton problem. He obtained the intrinsic equations of the curve ; and he shewed that one of its two branches, terminating in a cusp, satisfied the further criterion which he had devised. Thus all the conditions, then known as requisite for the possession of a minimum, were satisfied. Yet, in the same memoir, Legendre constructed a curve, made up of two straight lines, for which the integral expression for the resistance acquires a value less than the preceding supposed minimum. He also points out that the same kind of reduction of the supposed minimum can be obtained by taking a particular line, continuous and jagged, lying (as to its abscissæ) between any two points on the curve. This jagged continuous line does, in fact, contain the secret of the paradox. But Legendre did not recognise its full analytical significance: and he merely declared that such minima are "relative or accidental."

The explanation of the paradox is that the value, obtained as satisfying the criteria then known, is not a true minimum. There is a further test, with which the jagged line is essentially connected; and the Newton solid of revolution does not satisfy the test. This further criterion was devised by Weierstrass, apparently sometime about 1872, and was given in his lectures at Berlin in that year ; but he does not appear to have published his results in any other form.

3. Newton recognised that the question had a direct bearing upon the form of ships. In his day, the methods of propulsion were effectively limited to three : haulage, of minor importance ; the pressure of wind upon sails ; and the leverage use of oars, with the blade as fulcrum. But now, the agencies

---

[*] *Book* ii, *Prop*. xxxv (numbered xxxiv in the second edition), *Theorema* xxviii ; in the third part of the *Scholium*.

[†] "Mémoire sur la manière de distinguer les maxima des minima dans le Calcul des Variations," *Mém. de l'Acad. Roy. des Sciences* (1786), pp. 7-37. Euler, who originated the calculus, died in 1784.

of propulsion are mainly internal to the moving body; and, through the development of the airship and the submarine (vessel and torpedo), the problem has an added importance. An examination, and a revision, of the postulates of the problem propounded by Newton are therefore desirable. These postulates are three in number:

(i) that the solid is a body of revolution moving with uniform velocity, without rotation, in the direction of its axis;
(ii) that the resistance to motion is due to pressure alone;
(iii) that the resistance varies as the square of the velocity.

4. Of these postulates, the first is of a descriptive character only. It is the simplest hypothesis, suitable to the circumstances. It accords with fact, in respect of projectiles whether through air or water. Though it is hardly even a rough approximation to the form of ships, whether in air or under water, it still remains the hypothesis which, admitting of treatment by calculation, may lead to results that are suggestive. If such results can be attained, other forms of cross section are similarly amenable to calculations: if not, the more complicated calculations do not promise guidance.

The second postulate is in grave disagreement with both observation and experiment. For instance, it is estimated that, in the case of cruisers and destroyers, the resistance due to friction can be as much as 80 to 90 per cent. of the whole resistance.

The third postulate is fairly accurate over a large range of observation and experiment. It can be based upon the assumption that the medium is a perfect fluid; it ignores—what now are possibilities—the grave disturbance caused in the medium by the action of propellers; and it is far from being universal or even general as a descriptive law of fact. Thus for some moderate velocities and for some high velocities (using the terms now current for submarines and air-craft), an interpreting law can be taken that, for a velocity v, the resistance varies as $v^{1\cdot 83}$. If the law be assumed that the resistance varies as v^n, the number n can be less than 1·83 for low speeds: it can mount up to 3 and beyond, through continuous values to 4, for high speeds: and, for some high speeds, there are observations which indicate a value as large as 5.

All attempts in the interpretation of results have aimed at securing a monomial law: that is, only a single power of v is admitted into the expression of the law. There does not appear to be any hypothetical formulation of a law where the resistance varies as $v^n(1+cv^m)$, or of any law other than monomial.*

USE OF THE CALCULUS OF VARIATIONS.

5. The calculus of variations was unknown when Newton's *Principia* was published: the earliest result was obtained by Euler in 1744. The ancient Greek isoperimetrical problems, associated with Zenodorus and others, have long been used as staple examples of the subject. Newton's solid of least resistance is the earliest modern problem amenable to treatment by that calculus.

Four customary tests are exacted from an integral, if it is to possess a maximum or a minimum. Ranged in the sequence of their historical discovery, they are:

(*a*) The Euler (or Euler-Lagrange) test, constituted by the characteristic differential equation which leads to the determination of the dependent variable;
(*b*) The Legendre test;
(*c*) The Jacobi test;
(*d*) The Weierstrass test.

---

* Results and tables are given in Sir W. H. White's address to the Engineering Section of the British Association (in 1899, at Dover), and in various *Reports and Memoranda* published by H.M. Stationery Office for the Aeronautical Research Committee.

Of these, the first and the last are connected with the small terms of the first order in the expansion of the integral when the contained variables are subjected to arbitrary small variations. The Euler test is concerned with gentle (or *weak*) variations: the functions, which occur, are deemed expansible in powers of the small quantities stamping the smallness of the variations. With the usual notion of a maximum or a minimum, the terms of the first order must vanish while the terms of the second order must preserve a uniform sign (positive, giving an increase from a minimum: negative, giving a decrease from a maximum). The test, that the terms of the first order under gentle variations shall vanish, determines the dependent variable as a function of the independent variable. The relation between the two variables is described geometrically as the characteristic curve.

The Weierstrass test is concerned with abrupt (or *strong*) variations. It helps to decide whether the characteristic curve provides a maximum, or a minimum, or a mere stationary value which is neither a maximum nor a minimum. In practice, it usually is based upon the effect of a single strong variation of selected type; but, as it is cumulative in its nature, an extensive class of strong variations can be compounded of elements of that selected type. It is to be noted, however, that these composite strong variations do not constitute the aggregate of imaginable strong small variations.

The Legendre test and the Jacobi test affect the set of terms of the second order which arise when gentle variations are imposed upon the integral. They help to decide whether the characteristic curve provides a maximum or a minimum, and (if either a maximum or a minimum is provided) they settle the stretch of that curve along which the property is possessed. The Legendre test is qualitative at every place on the curve. The Jacobi test is quantitative; and it settles the limit (if any) to the range of the curve within which the integral acquires the maximum value or the minimum value.

Thus, in the discussion of any problem, the Euler test is the first to be applied, so that the central characteristic curve may be obtained. Owing to the historical development of the subject, the Legendre test is usually the next to be applied: then the Jacobi test, analytically cognate with the Legendre test, though first constructed at a later date (1837).

The Weierstrass test was constructed long after the Jacobi test. If, however, its intrinsic significance be separated from the date of its discovery, it can fittingly be made to follow the Euler test at once, before the others are applied; for it is concerned with terms of the first order alone, while they are concerned with terms of the second order alone.

The second-order tests are frequently satisfied when the Weierstrass first-order test definitely is not satisfied. In that event, the aggregate requisite tests are not satisfied: the problem then does not admit a true maximum or a true minimum.

Such is the fact for Newton's problem of the solid of least resistance, which now may be considered in detail.

Expression for the Total Resistance.

6. We assume that, alike for pressure and for friction, *the resistance varies as the n^{th} power of the speed.*

Let C be the absolute constant for pressure-resistance per unit of area, and C' the absolute constant for friction-resistance. Let ... CPC' ... represent the curve which, by rotation round the axis OA, generates the solid of the problem; let PT be the tangent at P, $PTO=\psi$.

The resultant resistance due to resisting pressure is

$$\int C \,.\, 2\pi x\, ds \,.\, v^n \cos^n \psi \,.\, \cos\psi\,;$$

and the resultant resistance due to resisting friction is

$$\int C' \,.\, 2\pi x \, ds \,.\, v^n \sin^n \psi \,.\, \sin \psi .$$

Let $C' = aC$, where a is a positive constant, which may depend upon v and is a subject for experiment. The whole resistance is

$$C\, 2\pi v^n \int x\, (\cos^{n+1} \psi + a \sin^{n+1} \psi)\, ds.$$

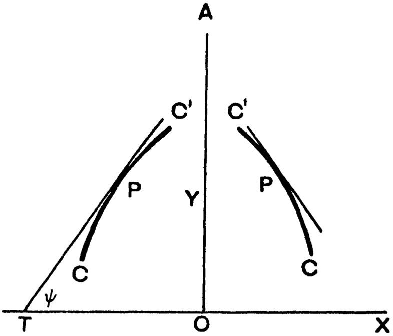


FIG. 1.

We take the coordinates of any point P on the curve to be functions of an independent variable t; and we write x_1 and y_1 for the first derivatives of x and y, so that

$$\frac{ds}{dt} = (x_1^2 + y_1^2)^{\frac{1}{2}}, \quad \cos \psi = x_1 (x_1^2 + y_1^2)^{-\frac{1}{2}}, \quad \sin \psi = y_1 (x_1^2 + y_1^2)^{-\frac{1}{2}},$$

where the positive sign is ascribed to the radical, in order that the arc may increase continually in length along the curve. Then the expression for the whole resistance becomes

$$C \,.\, 2\pi v^n \int F\, dt,$$

where
$$F = \frac{x_1^{n+1} + a y_1^{n+1}}{(x_1^2 + y_1^2)^{\frac{1}{2} n}}\, x.$$

In this expression, a is a positive constant. The resistance due to pressure alone will be given by making a zero; and the Newton problem then arises by taking $n = 2$. The resistance due to friction alone will be obtained by taking only the term in the numerator of F which involves a.

In order to have the solution of the problem, the integral

$$\int F\, dt$$

must be made a minimum. Accordingly, it must satisfy the four tests in succession.

EULER TEST: THE GENERAL EQUATION OF THE CURVE.

7. There are two characteristic equations

$$\frac{\partial F}{\partial x} - \frac{d}{dt}\left(\frac{\partial F}{\partial x_1}\right) = 0, \quad \frac{\partial F}{\partial y} - \frac{d}{dt}\left(\frac{\partial F}{\partial y_1}\right) = 0,$$

which are equivalent to a single equation, through the identity

$$x_1 \frac{\partial F}{\partial x_1} + y_1 \frac{\partial F}{\partial y_1} = F$$

satisfied by F. As y does not occur explicitly in F, the quantity $\frac{\partial F}{\partial y}$ is zero. Consequently, a first integral of the second of the equivalent equations is

$$\frac{\partial F}{\partial y_1} = c,$$

where c is an arbitrary constant of integration: that is,

$$x(x_1{}^2 + y_1{}^2)^{-(\frac{1}{2}n+1)} \{a y_1{}^n (x_1{}^2 + y_1{}^2) + n a y_1{}^n x_1{}^2 - n x_1{}^{n+1} y_1\} = c,$$

This is an equation for the characteristic curve. When the expressions for $\cos \psi$ and $\sin \psi$ are used, it takes the form

$$x\{a(1 + n \cos^2 \psi) \sin^n \psi - n \cos^{n+1} \psi \sin \psi\} = c,$$

which is an intrinsic equation of the curve. Also

$$\frac{dy}{d\psi} = -\frac{dx}{d\psi} \tan \psi = -\frac{ncN \sin \psi}{\{a(1 + n \cos^2 \psi) \sin^n \psi - n \cos^{n+1} \psi \sin \psi\}^2},$$

where

$$\begin{aligned} N &= a\{(n+1) \cos^2 \psi - \sin^2 \psi\} \sin^{n-1} \psi + \{(n+1) \sin^2 \psi - \cos^2 \psi\} \cos^{n-1} \psi \\ &= \cos^{n+1} \psi \{a \tan^{n-1} \psi (n + 1 - \tan^2 \psi) + (n+1) \tan^2 \psi - 1\}. \end{aligned}$$

The explicit quadrature of y, so as to obtain the expression for y as a function of ψ, does not appear possible for a quite general value of n. Manifestly, it will be of the form

$$y = y_0 + cf(\psi),$$

where y_0 is an arbitrary constant of integration, and the function $f(\psi)$ does not involve c.

LEGENDRE TEST.

8. It is easy to verify for the function F, as for all functions satisfying the same identity as F, that the relations

$$\frac{1}{x_1{}^2} \frac{\partial^2 F}{\partial y_1{}^2} = -\frac{1}{x_1 y_1} \frac{\partial^2 F}{\partial x_1 \partial y_1} = \frac{1}{y_1{}^2} \frac{\partial^2 F}{\partial x_1{}^2}$$

are satisfied. Let the common value of these three fractions be denoted by P, so that

$$P = \frac{nx}{(x_1{}^2 + y_1{}^2)^{2+\frac{1}{2}n}} \{-(x_1{}^{n+1} + a y_1{}^{n+1}) + (n+1)(x_1{}^{n-1} y_1{}^2 + a x_1{}^2 y_1{}^{n-1})\}.$$

The Legendre test requires that the quantity P shall not change its sign anywhere along the range of the curve, and that the sign must be positive for a minimum. Now

$$P \left(\frac{ds}{dt}\right)^3 = nxN.$$

Throughout the investigation, a positive sign is affixed to the radical $\frac{ds}{dt}$. The quantity x, being the actual distance of a point on the generating curve from the axis of revolution, is not negative. Hence *the quantity N must not vanish in the course of the range*; and limits of the range can be marked by zeroes of N.

If N vanishes, then $\frac{dx}{d\psi} = 0, \quad \frac{dy}{d\psi} = 0.$

For such values, the point generally is a cusp, the direction of the cuspidal tangent being given by that value of ψ which has made N vanish; thus the range of the curve is bounded by a cusp (if any) and must extend only along one branch of the curve terminating in that cusp.

9. The critical equation $N=0$ must therefore be considered. As the value of the positive constant a is not specified, and as the explicit quadrature for y for a general value of n cannot be effected, we must limit the discussion to illustrative instances.

(A). *Let* $n=1$. The equation $N=0$ is

$$2t^2-1+a(2-t^2)=0,$$

where t denotes $\tan\psi$: and the only root is

$$t^2=\frac{1-2a}{2-a}.$$

If $\frac{1}{2}\leqslant a\leqslant 2$, the quantity N is positive; and the Legendre test is satisfied, without any restriction on ψ.

If $0\leqslant a<\frac{1}{2}$, so that we may take $a=\frac{1}{2}-a'$, where a' is positive but not zero, the Legendre test is satisfied, provided $(3+2a')\tan^2\psi>4a'$.

If $a>2$, so that we may take $a=2+a''$, where a'' is positive but not zero, the Legendre test is satisfied, provided $t^2<2+\dfrac{3}{a''}$.

(B). *Let* $n=2$. The equation $N=0$ is

$$at(3-t^2)+3t^2-1=0,$$

that is,

$$a=\frac{1-3t^2}{3t-t^3}=\cot 3\psi.$$

The quantity a is real and positive; let $a=\tan\alpha$, where $0<\alpha<\frac{1}{2}\pi$, the value $\alpha=0$ being admissible when $a=0$: that is, for the Newton problem as originally propounded.

When a, being positive, is not zero, the roots t of N lie between the values $-\infty$, $-3^{-\frac{1}{2}}$, $3^{\frac{1}{2}}$, $+\infty$; and they are given by

$$3\psi=\tfrac{5}{2}\pi-\alpha,\quad \tfrac{3}{2}\pi-\alpha.\quad \tfrac{1}{2}\pi-\alpha.$$

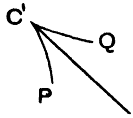


FIG. 2.

When a is zero, as for the Newton problem, the roots t of N are $\pm 3^{-\frac{1}{2}}$: and in order that N may be positive, $3t^2>1$. In this case, there is a cusp at C'; along $C'Q$ we have $3t^2<1$ and along $C'P$ we have $3t^2>1$. The branch $C'P$ satisfies the Legendre test.

(C). *Let* $n=3$. The equation $N=0$ is

$$at^2(4-t^2)+4t^2-1=0.$$

As a is a positive quantity, the roots of this quadratic in t^2 are real and positive; one of these roots lies between 0 and $\frac{1}{4}$, and the other is greater than 4. Denoting them respectively by T_1 and T_2, we have

$$N=a(T_2-\tan^2\psi)(\tan^2\psi-T_1),$$

and

$$P=3ax\cos^3\psi(T_2-\tan^2\psi)(\tan^2\psi-T_1).$$

Legendre's test is satisfied if $T_1<\tan^2\psi<T_2$.

When a is zero: that is, when the pressure-resistance alone is taken into account: there is a cusp, with $\tan^2\psi_0=\frac{1}{4}$. The branch of the curve along which $\tan^2\psi>\frac{1}{4}$, satisfies the Legendre test.

THE JACOBI TEST.

10. This test is concerned with the limits of a range along a characteristic curve within which the maximum or the minimum property can be possessed. (A simple example of limitation in range is provided by the shortest distance

between two points measured along the surface of a sphere; the arc of the great circle must be less than half the circle.)

In general, the explicit application of the Jacobi test cannot be made until the full primitive of the characteristic equations has been obtained: that is, when the general nature of the characteristic curve is known. The analytical formulation of the test is made from the primitive, before assigned conditions impose the precision suited to the particular problem.

In the present instance, we have

$$x = \frac{c}{a \sin^n \psi (1 + n \cos^2 \psi) - n \cos^{n+1} \psi \sin \psi} = \frac{c}{D};$$

and

$$\frac{dy}{d\psi} = \frac{dx}{d\psi} \tan \psi = -c \frac{D'}{D^2} \tan \psi,$$

so that

$$y = y_0 - c \int \frac{D'}{D^2} \tan \psi \, d\psi,$$

where c and y_0 are the arbitrary constants for the primitive. Thus

$$\frac{dx}{d\psi} = -c \frac{D'}{D^2}, \qquad \frac{\partial x}{\partial c} = \frac{1}{D}, \qquad \frac{\partial x}{\partial y_0} = 0,$$

$$\frac{dy}{d\psi} = -c \frac{D'}{D^2} \tan \psi, \quad \frac{\partial y}{\partial c} = -\int \frac{D'}{D^2} \tan \psi \, d\psi, \quad \frac{\partial y}{\partial y_0} = 1.$$

The functions, used for the Jacobi test, are

$$\theta_1 = \frac{dy}{d\psi} \frac{\partial x}{\partial c} - \frac{dx}{d\psi} \frac{\partial y}{\partial c}, \quad \theta_2 = \frac{dy}{d\psi} \frac{\partial x}{\partial y_0} - \frac{dx}{d\psi} \frac{\partial y}{\partial y_0};$$

and the critical function Θ is

$$\begin{aligned} \Theta = \frac{\theta_1}{\theta_2} &= \frac{\partial y}{\partial c} - \frac{\partial x}{\partial c} \tan \psi \\ &= -\int \frac{D'}{D^2} \tan \psi \, d\psi - \frac{1}{D} \tan \psi \\ &= -\int \frac{\sec^2 \psi}{D} \, d\psi. \end{aligned}$$

The Jacobi test requires that a range of integration for ψ, beginning with a value ψ_0, shall not extend as far as the value ψ_1, where Θ first resumes the value which it has at the initial place ψ_0.

Now

$$\frac{d\Theta}{d\psi} = -\frac{\sec^2 \psi}{D}.$$

So long as D does not vanish and change sign, $\frac{d\Theta}{d\psi}$ is uniform in sign; and, for that range, Θ is continually increasing or continually decreasing. Thus Θ can resume an initial value, after having passed through an infinite value and suffered a violent discontinuity. If, therefore, that initial value at ψ_0 is resumed at a value ψ_1, this conjugate value is given by the equation

$$\int_{\psi_0}^{\psi_1} \frac{\sec^2 \psi}{D} \, d\psi = 0.$$

For a general value of n, the quadrature does not admit of explicit expression in terms of known functions. It can be effected explicitly for particular values $n = 1, 2, 3, \dots$, in terms of logarithmic and circular functions; but for such values, the results are not of primary importance, because the one remaining test is not satisfied.

THE WEIERSTRASS TEST.

11. In the integral $\int F\,dt$, required to satisfy the conditions for the possession of a minimum, the subject of integration $F(x_1, y_1)$ is

$$F(x_1, y_1) = x\frac{x_1^{n+1} + a y_1^{n+1}}{(x_1^2 + y_1^2)^{\frac{1}{2}n}}.$$

Thus
$$\frac{\partial F(x_1, y_1)}{\partial x_1} = \frac{x}{(x_1^2 + y_1^2)^{1+\frac{1}{2}n}}\{x_1^{n+2} + (n+1)x_1^n y_1^2 - n a x_1 y_1^{n+1}\},$$

$$\frac{\partial F(x_1, y_1)}{\partial y_1} = \frac{x}{(x_1^2 + y_1^2)^{1+\frac{1}{2}n}}\{a y_1^{n+2} + (n+1) a x_1^2 y_1^n - n x_1^{n+1} y_1\}.$$

The critical Weierstrass function $\mathfrak{E}$ is

$$\mathfrak{E} = F(\lambda, \mu) - \lambda\frac{\partial F(x_1, y_1)}{\partial x_1} - \mu\frac{\partial F(x_1, y_1)}{\partial y_1}.$$

To use this function, we take the independent variable t to be the length s of the arc of the characteristic curve from any point, so that

$$x_1 = \cos\psi, \quad y_1 = \sin\psi.$$

The quantities λ and μ are direction-cosines of any direction through any and every freely-selected point x, y on the curve, so that $\lambda^2 + \mu^2 = 1$. If, then, we take

$$\mathfrak{E} = x E_n,$$

the quantity x not changing its sign (it is merely the distance of a point on the generating curve from the axis of revolution), we have

$$\begin{aligned} E_n = {} & \lambda^{n+1} + a\mu^{n+1} \\ & - \lambda\{\cos^n\psi + n\cos^n\psi\sin^2\psi - na\cos\psi\sin^{n+1}\psi\} \\ & - \mu\{a\sin^n\psi + na\cos^2\psi\sin^n\psi - n\cos^{n+1}\psi\sin\psi\}. \end{aligned}$$

The Weierstrass test requires that, for the existence of a minimum or a maximum, *the function $\mathfrak{E}$ shall keep the same sign, at all places on the characteristic curve, and for all directions λ and μ through each place which do not coincide with the tangent.* For the existence of a minimum, this uniform sign must be positive. Consequently, the test in the present case is that the function E_n shall be positive, at every place on the curve, and for all directions λ and μ where λ differs from $\cos\psi$ and μ from $\sin\psi$.

12. One general proposition follows at once from the use of the Weierstrass test: an integral $\int F(x, y, x_1, y_1)\,dt$ cannot have a maximum or a minimum when $F(x, y, x_1, y_1)$ is a rational function of its arguments x_1 and y_1. This result is immediately obvious for the present function F; for then x must be an even integer, in which event

$$E_n(-\lambda, -\mu) = -E_n(\lambda, \mu),$$

that is, E_n changes its sign by a reversal of the arbitrary direction. Thus in particular, when $n = 2$, there is no minimum, whatever be the value of a: in general, *when n is an even integer, there is no minimum.*

THE NEWTON PROBLEM.

13. (A). Owing to the historical interest of the Newton problem, it is worth while deducing this result in a more special form, even when the term involving a is not neglected. When $n = 2$, we have

$$\begin{aligned} E_2 = {} & \lambda^3 + a\mu^3 \\ & - \lambda(\cos^2\psi + 2\cos^2\psi\sin^2\psi - 2a\cos\psi\sin^3\psi) \\ & - \mu(-2\cos^3\psi\sin\psi + a\sin^2\psi + 2a\sin^2\psi\cos^2\psi). \end{aligned}$$

Let χ denote the (non-zero) angle between the arbitrary direction λ and μ, and the tangent to the characteristic curve, so that $\lambda = \cos(\psi+\chi)$ and $\mu = \sin(\psi+\chi)$. Then, as is easily verified,

$$\lambda^3 - \lambda(\cos^2\psi + 2\cos^2\psi\sin^2\psi) + 2\mu\cos^3\psi\sin\psi = -\sin^2\chi\cos(3\psi+\chi),$$

$$\mu^3 - \mu(\sin^2\psi + 2\sin^2\psi\cos^2\psi) + 2\lambda\sin^3\psi\cos\psi = \sin^2\chi\sin(3\psi+\chi).$$

Hence $$E_2 = -\{\cos(3\psi+\chi) - a\sin(3\psi+\chi)\}\sin^2\chi.$$

As a is a finite positive quantity, let $a = \tan\alpha$, where $0 < \alpha < \frac{1}{2}\pi$; then

$$E_2 = -\sin^2\chi\sec\alpha\cos(3\psi+\alpha+\chi).$$

Manifestly * as χ ranges from 0 to 2π, the function E_2 changes sign. The Weierstrass test is not satisfied: there is no minimum.

Hence, *when the resistance is taken to vary as the square of the velocity, there is no solution of the problem to determine the shape of a solid of revolution undergoing a minimum of resistance, whether friction be taken into account or be ignored.*

LAWS OF RESISTANCE DIFFERENT FROM NEWTON'S LAW OF THE SQUARE.

14. When the resistance varies as the fourth power of the velocity the same result holds: there is no solid of revolution undergoing a minimum of resistance.

There remains an enquiry as to the possibility of such a solid for other laws v^n. We shall discuss solely the two cases:—$n=1$, for low velocities; and $n=3$, for some high velocities (as these are estimated at the present time).

15. (B). *Let* $n=1$. We have seen that the Legendre test is satisfied

(α) when $\frac{1}{2} \leqslant a \leqslant 2$, without conditions;

(β) when $0 \leqslant a < \frac{1}{2}$, with limitations on the value of $\tan^2\psi$;

(γ) when $2 < a$, with limitations on the value of $\tan^2\psi$.

For E_1, as the essential factor in the Weierstrass function $\mathfrak{E}$, we have

$$\begin{aligned} E_1 = \lambda^2 + a\mu^2 & \\ & - \lambda(\cos\psi + \cos\psi\sin^2\psi - a\cos\psi\sin^2\psi) \\ & - \mu(-\cos^2\psi\sin\psi + a\sin\psi + a\cos^2\psi\sin\psi). \end{aligned}$$

If, as before, we take $\lambda = \cos(\psi+\chi)$ and $\mu = \sin(\psi+\chi)$, so that χ is the inclination of the arbitrary direction λ and μ to the tangent, then

$$\begin{aligned} \lambda^2 - \lambda(\cos\psi + \cos\psi\sin^2\psi) + \mu\cos^2\psi\sin\psi & \\ = \tfrac{1}{2}(1-\cos\chi)\{1 - \cos 2\psi - 2\cos(2\psi+\chi)\}, & \\ \mu^2 - \mu(\sin\psi + \sin\psi\cos^2\psi) + \lambda\sin^2\psi\cos\psi & \\ = \tfrac{1}{2}(1-\cos\chi)\{1 + \cos 2\psi + 2\cos(2\psi+\chi)\}. & \end{aligned}$$

We write $$G(\chi) = \cos 2\psi + 2\cos(2\psi+\chi);$$

and now $$E_1 = \tfrac{1}{2}(1-\cos\chi)\{1 + a - (1-a)G(\chi)\}.$$

The Weierstrass test is satisfied if E_1 never changes sign; but it is not satisfied if E_1 can vanish and change sign. The value $\chi = 0$ is to be ignored: it would imply a direction coincident with the tangent, giving rise to a non-zero deviation. We must therefore consider the function $1 + a - (1-a)G(\chi)$, which will be denoted by $f(\chi)$. Now

$$f(-2\psi) = 3a - 1 - (1-a)\cos 2\psi,$$

$$f(\pi - 2\psi) = 3 - a - (1-a)\cos 2\psi.$$

---

* The result for $a=0$ (the Newton Problem) was first obtained by Weierstrass.

(i) When $0 \leqslant a < \frac{1}{2}$, the Legendre test requires that

$$\tan^2 \psi > \frac{1-2a}{2-a} = \frac{1-2a}{2-a} + \mu,$$

where μ is positive. Then

$$f(-2\psi) = \frac{2}{3-3a+\mu(2-a)} \{a(2-a)\mu - 2(1-a)(1-2a)\},$$

so that there certainly exist positive values of μ such that $f(-2\psi)$ is negative. The function $f(\pi - 2\psi)$ is always positive for the specified range of a. Thus there is some value of χ lying between $2\pi - 2\psi$ and $\pi - 2\psi$, where $f(\chi)$ vanishes and changes sign certainly for some values of ψ. Thus the Weierstrass test is not satisfied if $0 \leqslant a < \frac{1}{2}$; and there is no minimum for the integral.

In particular, if $a=0$ when the pressure resistance alone is considered, there is no minimum: that is, when the resistance varies as the first power of the velocity.

(ii) When $a > 2$, the Legendre test requires that

$$\tan^2 \psi < \frac{2a-1}{a-2} = \frac{2a-1}{a-2} - \lambda,$$

where λ is positive. Then as $a - 1 < 3a - 1$, the function $f(-2\psi)$ is always positive. Also

$$f(\pi - 2\psi) = \frac{2(a-2)^2}{\sec^2 \psi} \left\{\lambda - 2\frac{a-1}{a-2}\right\},$$

so that there certainly exist positive values of λ such that $f(\pi - 2\psi)$ is negative. Thus there is some value of χ lying between $2\pi - 2\psi$ and $\pi - 2\psi$, where $f(\chi)$ vanishes and changes sign certainly for some values of ψ. Thus the Weierstrass test is not satisfied if $a > 2$; and there is no minimum for the integral.

In particular, if a be very large, *when the friction resistance alone need be considered, there is no minimum: that is, when the resistance varies as the first power of the velocity.*

(iii) When $\frac{1}{2} \leqslant a \leqslant 2$, the Legendre test is satisfied without any restriction on the values of ψ. We have

$$\begin{aligned} G(\chi) &= \cos 2\psi + 2\cos(2\psi + \chi) \\ &= (5 + 4\cos\chi)^{\frac{1}{2}} \cos(2\psi + \theta), \end{aligned}$$

where

$$\tan\theta = \frac{2\sin\chi}{1 + 2\cos\chi}:$$

so that as χ never can be zero for the present quest, the numerical value of $G(\chi)$ is less than 3.

Let $a = \frac{1}{2} + \mu$, so that μ may range from 0 to $\frac{3}{2}$. Now

$$\begin{aligned} E_1 &= 1 + a - (1-a)G(\chi) \\ &= (\tfrac{3}{2} + \mu)\left\{1 - \frac{1-2\mu}{3+2\mu} G(\chi)\right\}. \end{aligned}$$

As μ ranges from 0 to $\frac{1}{2}$, the quantity E_1 remains positive. If $\mu = \frac{1}{2}$, or if $\mu = \frac{3}{2}$, the quantity E_1 is positive. As μ ranges from $\frac{1}{2}$ to $\frac{3}{2}$, the quantity

$$1 + \frac{2\mu - 1}{2\mu + 3} G(\chi)$$

remains positive. Thus E_1 does not change its sign. The Weierstrass test is satisfied.

There can be a minimum for the integral if a lies between the values $\frac{1}{2}$ and 2, the law of resistance being the first power of the velocity.

In particular, if $a=1$, all the three conditions, represented by the Euler test, the Legendre test, and the Weierstrass test, are satisfied. The generating curve is

$$\frac{x}{c}=\cosh\frac{y-y_0}{c},$$

a catenary. The Jacobi test, in the form customary for the catenary, then requires that a range on the curve beginning at a point A shall not extend as far as the point B, where the tangents at A and B intersect on the directrix.

Thus *the value of a, the ratio of the constants of friction resistance to the constant of pressure resistance when the law of resistance varies as the first power, is required. If experiment shews that it can lie between $\frac{1}{2}$ and 2, there can be a solid of minimum resistance: not otherwise.*

16. (C). *Let $n=3$*: so that the resistance varies as the cube of the velocity, a result according with observations on cruisers at fairly high speeds. For the Legendre test, we have

$$\begin{aligned}P_3\left(\frac{ds}{dt}\right)^3&=3x(4\cos^2\psi\sin^2\psi+4a\cos^2\psi\sin^2\psi-\cos^4\psi-a\sin^4\psi)\\&=3x\cos^4\psi\,\{4(1+a)t^2-1-at^4\}\\&=3xa\cos^4\psi\,(t_1{}^2-t^2)(t^2-t_2{}^2),\end{aligned}$$

where t denotes $\tan\psi$, and $t_1{}^2(>t_2{}^2)$ and $t_2{}^2$ are the two roots, real and positive, of the equation

$$az^2-4(1+a)z+1=0.$$

For the Weierstrass test, we have

$$\begin{aligned}E_3=\lambda^4&+a\mu^4\\&-\lambda(\cos^3\psi+3\cos^3\psi\sin^2\psi-3a\cos\psi\sin^4\psi)\\&-\mu(-3\cos^4\psi\sin\psi+a\sin^3\psi+3a\sin^3\psi\cos\psi).\end{aligned}$$

Now

$$\lambda^4-\lambda(\cos^3\psi+3\cos^3\psi\sin^2\psi)+3\mu\cos^4\psi\sin\psi=\tfrac{1}{2}(1-\cos\chi)\{J(\chi)-G(\chi)\},$$
$$\mu^4-\mu(\sin^3\psi+3\sin^3\psi\cos^2\psi)+3\lambda\sin^4\psi\cos\psi=\tfrac{1}{2}(1-\cos\chi)\{J(\chi)+G(\chi)\},$$

where

$$J(\chi)=\tfrac{3}{4}-\tfrac{3}{4}\cos 4\psi-\tfrac{3}{2}\cos(4\psi+\chi)-\cos(4\psi+2\chi)-\tfrac{1}{2}\cos(4\psi+3\chi),$$
$$G(\chi)=\cos 2\psi+2\cos(2\psi+\chi).$$

Hence
$$\frac{E_3}{\frac{1}{2}(1-\cos\chi)}=J(\chi)-G(\chi)+a\{J(\chi)+G(\chi)\}.$$

17. Before the general form is discussed, two inferences can easily be derived:

(i) there is no solid of revolution for which the resistance, due to pressure alone, is a minimum; and

(ii) there is no solid of revolution for which the resistance, due to friction alone, is a minimum.

Both inferences are based on the Weierstrass test. We have

$$\left.\begin{aligned}J(-2\psi)&=-\tfrac{1}{4}-2\cos 2\psi-\tfrac{3}{4}\cos 4\psi\\J(\pi-2\psi)&=-\tfrac{1}{4}+2\cos 2\psi-\tfrac{3}{4}\cos 4\psi\\J(\pi)&=\tfrac{3}{4}+\tfrac{1}{4}\cos 4\psi\end{aligned}\right\},\quad\left.\begin{aligned}G(-2\psi)&=2+\cos 2\psi\\G(\pi-2\psi)&=-2+\cos 2\psi\\G(\pi)&=-\tfrac{1}{2}\cos 2\psi\end{aligned}\right\}.$$

Thus

$$J(-2\psi)-G(-2\psi)=-\tfrac{3}{4}(1+\cos 2\psi)^2,\quad J(\pi)-G(\pi)=\tfrac{1}{4}(1+\cos 2\psi)^2.$$

Consequently, for some value of χ lying between π and $2\pi-2\psi$, the function $J(\chi)-G(\chi)$ vanishes; and it changes its sign as χ passes through that value, so that the corresponding function E_3 does not maintain a uniform sign for all admissible values of χ and of ψ (though the values of ψ do not need to be taken into account). The function $J(\chi) - G(\chi)$ is critical for the estimate of the pressure-resistance alone. Hence the first inference follows.

Again,

$$J(\pi - 2\psi) + G(\pi - 2\psi) = -\tfrac{3}{4}(1 - \cos 2\psi)^2, \quad J(\pi) + G(\pi) = \tfrac{1}{4}(1 - \cos 2\psi)^2.$$

Consequently, for some value of χ lying between π and $\pi - 2\psi$, the function $J(\chi) + G(\chi)$ vanishes; and it changes its sign as χ passes through that value, so that the corresponding function E_3 does not maintain a uniform sign for all admissible values of χ and of ψ (though, as before, the values of ψ do not need to be taken into account). The function $J(\chi) + G(\chi)$ is critical for the estimate of the friction-resistance alone. Hence the second inference follows.

18. For the most general case when the whole resistance (from pressure and friction together) is taken into account, let

$$W(\chi) = \frac{4E_3}{1 - \cos\chi}.$$

Then we find

$$W(-2\psi) = 5a - 3 - (2a + 6)\cos 2\psi - (3 + 3a)\cos^2 2\psi,$$

$$W(\pi - 2\psi) = 5 - 3a + (2 + 6a)\cos 2\psi - (3 + 3a)\cos^2 2\psi.$$

Owing to the requirements of the Legendre test, the quantity

$$4(1 + a)t^2 - 1 - at^4,$$

which is equal to $$\frac{1}{a}4 + 7a + 8a^2 - \{at^2 - 2(1 + a)\}^2,$$

must be positive throughout the range: it has its greatest value when

$$at^2 = 2(1 + a),$$

and it remains positive for values of t^2 in the immediate vicinity of $2 + \frac{2}{a}$.

Now $$W(-2\psi) = \cos^4\psi(-12 + 16at^2 + 4at^4),$$

which is certainly positive for values of t^2 actually or nearly equal to $2 + \frac{2}{a}$; while

$$W(\pi - 2\psi) = \cos^4\psi(4 + 16t^2 - 12at^4),$$

which is certainly negative for those values of t^2. Hence for every point on the indicated range of the characteristic curve, there is a value of χ (lying between $\pi - 2\psi$ and $2\pi - 2\psi$ at the point) at which E_3 vanishes, while E_3 changes its sign as χ passes through the value. Thus the Weierstrass test is not satisfied: *there is no solid of revolution which undergoes a minimum resistance when the law of resistance varies as the cube of the velocity.*

As the Weierstrass test is not satisfied, an application of the Jacobi test is superfluous.

19. The same result—that there is no solid of revolution of minimum resistance—holds when $n = 6$ and when $n = 5$. The case $n = 6$ is included in the general result stated in § 12. The result for the case $n = 5$ follows from analysis similar to that in §§ 16–18.

The consideration of larger integer values for n seems unnecessary, so far as concerns experimental observations hitherto noted.

20. Throughout the investigation, the arbitrary constant c in the first integral of the characteristic equation has been tacitly assumed not to be zero. If c could be zero, then either $x = 0$, a result which leads only to a filament solid: or $\sin\psi = 0$, for which the Legendre test is not satisfied.

4 *October*, 1926.

NEWTON'S WORK ON THE THEORY OF THE TIDES.

By Prof. J. Proudman, D.Sc., F.R.S.

It is now well known that the tides of the ocean constitute a gravitational phenomenon in which the Moon plays a large part. Yet the great Galileo wrote:* "Amongst all the famous men who have philosophated upon this admirable effect of Nature, I more wonder at Kepler than any of the rest, who being of a free and piercing wit, and having the motion ascribed to the Earth, before him, hath for all that given his ear and assent to the Moon's predominancy over the Water, and to occult properties, and such like trifles." This quotation well illustrates the state of knowledge on the nature of the tides shortly before the time of Newton. It was the *Principia* that laid the foundation of all sound work on the subject.

PRINCIPIA.

Book I., Proposition 66.

Section XI. of the *Principia* treats "of the motion of bodies tending to each other with centripetal forces,"† and Proposition 66 of this section treats of certain aspects of the problem of three bodies such that "the accelerative attractions of any two towards the third be between themselves reciprocally as the squares of the distances."

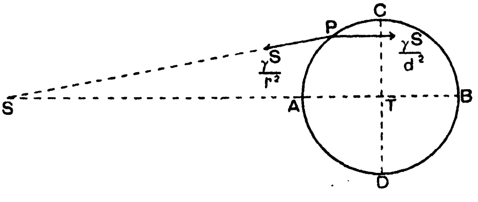


Fig. 1. ($d = ST$, $r = SP$.)

Fig. 1, without the arrows and associated magnitudes, is part of Newton's diagram for Proposition 66, the three bodies being S, T and P, which are supposed always to lie in one plane. We shall use these letters to denote also the masses of the corresponding bodies. Considering only motion relative to T, the force-intensity at P, in so far as it is due to S, is the vector-sum of the two force-intensities shown in Fig. 1, γ denoting the gravitation-constant. These two force-intensities are equivalent to the three shown in Fig. 2, and if the distance d is very great compared with the distance a, these force-intensities are easily seen to be approximately equivalent to those shown in

---

* Fourth dialogue of his *System of the World*; Salusbury's translation, quoted through R. A. Harris, *Manual of Tides*, I. p. 400.

† All quotations from the *Principia* in the present article are from Motte's translation (1st edit. 1729); the naming of the forces has been modified, and the spelling of a few words modernised.

Fig. 3. The presence of the factor d^3 in the denominators is brought out by Newton in *Corollary* 14.

By considering the component of acceleration transverse to *TP*, Newton deduces in *Corollary* 2 that "the body *P*, by the radius *TP*, will describe its area swifter near the conjunction *A* and the opposition *B* than it will near the quadratures *C* and *D*." Then follows *Corollary* 3 : "And from the same reasoning it appears that the body *P*, *caeteris paribus*, moves more swiftly in the conjunction and opposition than in the quadratures." Now

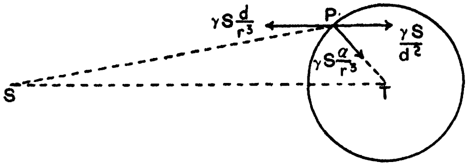


FIG. 2. ($a=TP$.)

$$(\text{speed})^2 \times (\text{curvature})$$

is equal to the inward normal component of the acceleration due to both *T* and *S* ; and this is greatest at *C* and *D* and least at *A* and *B*. Taking also into account *Corollary* 3, we deduce that "the orbit of the body *P*, *caeteris paribus*, is more curved at the quadratures than at the conjunction and opposition" (*Corollary* 4), and "Hence the body *P*, *caeteris paribus*, goes farther from the body *T* at the quadratures than at the conjunction and opposition" (*Corollary* 5).

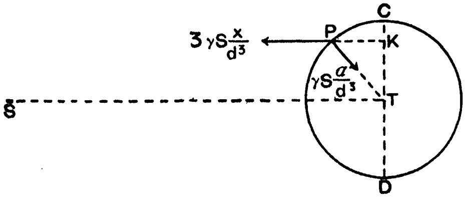


FIG. 3. ($x=KP$.)

If *M* denotes the periodic time of *P* round a fixed *T*, and *Y* the periodic time of *T* round a fixed *S*, we have

$$\frac{\gamma T}{a^2}=a\,\frac{4\pi^2}{M^2},\quad \frac{\gamma S}{d^2}=d\,.\,\frac{4\pi^2}{Y^2},$$

so that

$$\frac{S}{T}\frac{a^3}{d^3}=\frac{M^2}{Y^2},$$

a relation which is brought out by Newton in *Corollary* 17.

Now when, as in Book III., Proposition 22, *S*, *T* and *P* are interpreted as the Sun, Earth and Moon respectively, the *Corollaries* 2-5 give the great inequality in the Moon's motion, known as the *variation*, which was discovered observationally by Tycho Brahe just about a century earlier. But in *Corollary* 18 Newton proceeds : "By the same laws by which the body *P* revolves about the body *T*, let us suppose many fluid bodies to move round *T* at equal distances from it ; and to be so numerous that they may all become contiguous to each other, so as to form a fluid annulus or ring, of a round figure and concentrical to the body *T* ; and the several parts of this annulus . . . will draw nearer to the body *T* and move swifter in the conjunction and opposition . . . than in the quadratures." In *Corollary* 19 : "Suppose now the spherical body T, consisting of some matter not fluid, to be enlarged, and to extend itself on every side as far as that annulus, and that a channel were cut all round its circumference containing water ; and that this sphere revolves uniformly about its own axis in the same periodical time. This water being accelerated and retarded by turns (as in the last corollary) will be swifter at the syzygies, and slower at the quadratures than the surface of the globe, and so will ebb and flow in its channel after the manner of the sea." "And the force along PT will attract the water downwards at the quadratures, and depress it as far as the syzygies ; and the force along KP will attract it upwards in the syzygies, and withhold its descent, and make it rise as far as the quadratures ; except only in so far as the motion of flux and reflux may be directed by the channel of the water, and be a little retarded by friction." In *Corollary* 20 Newton considers a globe "a little higher or a little denser in the equatorial than in the polar regions," and says : "Yet the phenomena . . . of the preceding corollary would scarce be altered ; except that the places of greatest and least height of the water will be different. For the water is now no longer sustained and kept in its orbit by its centrifugal force, but by the channel in which it flows. And besides the force along PT attracts the water downwards most in the quadratures, and the force along KP attracts it upwards most in the syzygies. And these forces conjoined cease to attract the water downwards, and begin to attract it upwards in the octants before the syzygies ; and cease to attract the water upwards, and begin to attract the water downwards in the octants after the syzygies. And hence the greatest height of the water may happen about the octants after the syzygies ; and the least height about the octants after the quadratures ; excepting only so far as the motion of ascent and descent impressed by these forces may by the *vis insita* of the water continue a little longer, or be stopped a little sooner by impediments in its channel."

In reviewing the above we may first remark that the approximation of Fig. 3, when P denotes a particle of water on the Earth, is that which forms the basis of the construction of most of the present-day tide-tables. Of course the error has been investigated, but it is found to be of very small practical importance.

It is implied that the fluid ring of *Corollary* 18 passes near the line TS, and that the channel of *Corollary* 19 lies nearly in the equator. In *Corollary* 19 Newton indicates that his results may require modification on account of the direction of the water by its channel, and in *Corollary* 20 he points out that the pressure of the channel takes the place of the centrifugal force of corollaries earlier than 18. But he does not appear to have quite realised the extent of this modification, as indeed no other worker did until Laplace took up the subject just about a century after the publication of the *Principia*. For the pressure at the bottom of the channel balances the vertical component of the gravitating forces on the water and leaves the horizontal component as the sole tide-generating force. This vitiates the conclusions relative to the octants stated in *Corollary* 20. The conclusion of *Corollary* 19 regarding the ebb and flow is unaffected by the fact just mentioned, as it depends on *Corollaries* 2 and 3, in which only the acceleration-component tangential to the path was taken into account. But even in *Corollaries* 18 and 19 the consideration of a fluid, as distinct from discrete and separate particles, requires the discussion of dynamical pressure-gradients, a subject which indeed Newton takes up in other connections in Book II. of the *Principia*. However, a complete hydrodynamical investigation shows that for a uniform channel extending round the Equator, the *results* of *Corollary* 19 are entirely correct, providing the depth of water be less than about thirteen miles. For in such a channel the current would be easterly and the water lowest directly under and opposite the disturbing body, while the current would be westerly and the water highest where the disturbing body was rising or setting.

BOOK III., PROPOSITION 24.

Book III. of the *Principia* treats "of the system of the world," and Proposition 24 is to the effect "that the flux and reflux of the Sea, arise from the actions of the Sun and Moon."

Newton begins: "By Corollaries 19 and 20, Proposition 66, Book I., it appears that the waters of the sea ought twice to rise and twice to fall every day, as well lunar as solar; and that the greatest height of the waters in the open and deep seas, ought to follow the appulse of the luminaries to the meridian of the place, by a less interval than 6 hours; as happens in all that eastern tract of the *Atlantic* and *Æthiopic* seas between *France* and the *Cape of Good Hope*; and on the coasts of *Chili* and *Peru* in the *South-Sea*; in all which shores the flood falls out about the second, third or fourth hour, unless where the motion propagated from the deep ocean is by the shallowness of the channels, through which it passes to some particular places, retarded to the fifth, sixth or seventh hour, and even later." This statement regarding Atlantic (and Æthiopic) tides is true, except for a short stretch of the coast of Africa north of Liberia, where the interval changes from 6 hours to 12 hours. It is also true for the southern portion of the coast of Chili and for the greater part of the coast of Peru, and also in fact from Peru to Mexico.

Newton continues: "The force of the Sun or Moon in raising the sea, is greatest in the appulse of the luminary to the meridian of the place. But the force impressed upon the sea at that time continues a little while after the impression, and is afterwards increased by a new, though less, force still acting upon it. This makes the sea rise higher and higher, till this new force becoming too weak to raise it any more, the sea rises to its greatest height. And this will come to pass perhaps in one or two hours, but more frequently near the shores in about three hours, or even more where the sea is shallow." In the main this is erroneous, largely for the reason already mentioned regarding the non-efficacy of the vertical component of force.

"The two luminaries excite two motions, which will not appear distinctly, but between them will arise one mixed motion compounded out of both. In the conjunction or opposition of the luminaries their forces will be conjoined, and bring on the greatest flood and ebb. In the quadratures the Sun will raise the waters which the Moon depresses, and depress the waters which the Moon raises, and from the difference of their forces, the smallest of all tides will follow. And because (as experience tells us) the force of the Moon is greater than that of the Sun, the greatest height of the waters will happen about the" time of lunar tide. "Out of the syzygies and quadratures, the greatest tide, which by the single force of the Moon ought to fall out at" one time, "and by the single force of the Sun at" another time, "by the compounded forces of both must fall out in an intermediate time that approaches nearer to" that "of the Moon than to that of the Sun. And therefore while the Moon is passing from the syzygies to the quadratures . . . the greatest height of the waters will . . . precede" that due "to the Moon . . . and that by the greatest interval, a little while after the octants of the Moon; and by like intervals the greatest tide will follow" that due to the Moon, "while the Moon is passing from the quadratures to the syzygies. Thus it happens in the open sea. For in the mouths of rivers, the greatest tides will come later to their height." In this paragraph, which gives an accurate explanation of spring and neap tides with the associated phenomena of priming and lagging, the words not quoted simply replace "the third hour of the Moon" and "the third hour of the Sun," and this of course makes no difference to the argument.

"But the effects of the luminaries depend upon their distances from the Earth. For when they are less distant, their effects are greater, and when more distant, their effects are less, and that in the triplicate proportion of their apparent diameter. Therefore it is, that the Sun, in the winter time, being then in its perigee, has a greater effect, and makes the tides in the syzygies something greater, and those in the quadratures something less than in the summer season; and every month the Moon, while in the perigee, raises greater tides than at the distance of 15 days before or after, when it is in its apogee. Whence it comes to pass, that two highest tides don't follow, one the other, in two immediately succeeding syzygies."

"The effect of either luminary doth likewise depend upon its declination or distance from the equator. For, if the luminary was placed at the pole, it would constantly attract all the parts of the waters, without any intension or remission of its action, and could cause no reciprocation of motion. And therefore, as the luminaries decline from the equator towards either pole, they will, by degrees, lose their force, and on this account will excite lesser tides in the solstitial than in the equinoctial syzygies. But in the solstitial quadratures, they will raise greater tides than in the quadratures about the equinoxes; because the force of the Moon, then situated in the equator, most exceeds the force of the Sun. Therefore the greatest tides will fall out in those syzygies, and least in those quadratures, which happen about the time of both equinoxes: and the greatest tide in the syzygies is always succeeded by the least tide in the quadratures, as we find by experience. But, because the Sun is less distant from the Earth in winter than in summer, it comes to pass that the greatest and least tides more frequently appear before than after the vernal equinox, and more frequently after than before the autumnal." All this is correct.

"Moreover, the effects of the luminaries depend upon the latitudes of places. Let *ApEP* (Fig. 4) represent the Earth covered with deep waters; *C* its

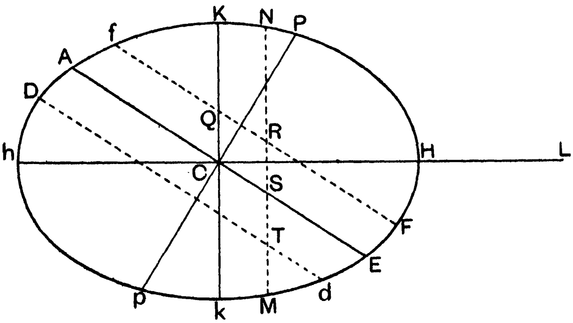


FIG. 4.

centre; *P*, *p* its poles; *AE* the equator; *F*, any place without the equator; *Ff*, the parallel of the place; *Dd*, the corresponding parallel on the other side of the equator; *L*, the place of the Moon three hours before; *H*, the place of the Earth directly under it; *h*, the opposite place; *K*, *k*, the places at 90° distance; *CH*, *ch*, the greatest heights of the sea from the centre of the Earth; *CK*, *ck*, its least heights: and if with the axes *Hh*, *Kk*, an ellipse is described, and by the revolution of that ellipse about its longer axis *Hh*, a spheroid *HPKhpk* is formed, this spheroid will nearly represent the figure of the sea; and *CF*, *cf*, *CD*, *cd* will represent the heights of the sea in the places *C*, *f*, *D*, *d*. But further, in the said revolution of the ellipse any point *N* describes the circle *NM*, cutting the parallels *Ff*, *Dd* in any places *R*, *T*; and the equator *AE* in *S*; *CN* will represent the height of the sea in all those places *R*, *S*, *T*, situated in this circle. Wherefore in the diurnal revolution of any place *F*, the greatest flood will be in *F* at the third hour after the appulse of the Moon to the meridian above the horizon; and afterwards the greatest ebb in Q . . . after the setting of the Moon ; and then the greatest flood in f at the third hour after the appulse of the Moon to the meridian under the horizon, and lastly, the greatest ebb at Q . . . after the rising of the Moon ; and the latter flood in f, will be less than the preceding flood in F."

We notice that the interval of three hours again appears, but as regards this interval the reasoning of the paragraph merely requires that it be the same for all points of an ocean covering the whole Earth. Now Laplace, who first gave a complete hydrodynamical treatment, showed that such is true provided the depth be a function solely of the latitude ; only the interval will be either zero or 6 hours. But Laplace also showed that the surface of the sea at any time will not in general be an ellipsoid of revolution. Nevertheless it is quite true that " thence arise tides, alternately greater and less . . . ; and when the Moon changes its declination to the other sides of the equator, that which was the greater tide will be changed into a lesser. And the greatest difference of the floods will fall out about the solstices ; especially if the ascending node of the Moon is about the first of Aries. So it is found by experience that " if " the morning tides in winter exceed those of the evening . . . the evening tides in summer exceed those of the morning ; at *Plymouth* by the height of one foot, but at *Bristol* by the height of 15 inches, according to the observations of *Colepress* and *Sturmy*."

" But the motions which we have been describing, suffer some alteration from that force of reciprocation, which the waters, being once moved, retain a little while by their *vis insita*." " And hence it is, that the alternate tides at *Plymouth* and *Bristol* don't differ much more one from the other than by the height of a foot or 15 inches, and that the greatest tides of all at those ports are not the first but the third after the syzygies. And besides all the motions are retarded in their passage through shallow channels, so that the greatest tides of all in some straits and mouths of rivers, are the fourth or even the fifth after the syzygies."

The only important factor not mentioned by Newton is the dynamical effect of the Earth's rotation.

" Farther it may happen that the tide may be propagated from the ocean through different channels towards the same port, and may pass quicker through some channels than through others, in which case the same tide, divided into two or more succeeding one another, may compound new motions of different kinds. Let us suppose two equal tides flowing towards the same port from different places, the one preceding the other by 6 hours ; and suppose the first tide to happen at the third hour of the appulse of the Moon to the meridian of the port. If the Moon at the time of the appulse to the meridian was in the equator, every six hours alternately there would arise equal floods, which meeting with as many equal ebbs would so balance one the other, that for that day the water would stagnate and remain quiet. If the Moon then declined from the equator, the tides in the ocean would be alternately greater and less as was said. And from thence two greater and two lesser tides would be alternately propagated towards that port. But the two greater floods would make the greatest height of the water to fall out in the middle time between both ; and the greater and lesser floods would make the waters to rise to a mean height in the middle time between them, and in the middle time between the two lesser floods the waters would rise to their least height. Thus in the space of 24 hours the waters would come, not twice, as commonly, but once only to their greatest, and once only to their least height ; and their greatest height, if the Moon declined towards the elevated pole, would happen at the 6th or 30th hour after the appulse of the Moon to the meridian ; and when the Moon changed its declination this flood would be changed into an ebb. An example of all which Dr. Halley has given us, from the observations of seamen in the port of *Batsham* in the kingdom of *Tonquin* in the latitude of 20° 50′ north. In that port, on the day which follows after the passage of the Moon over the equator, the waters stagnate: when the Moon declines to the north they begin to flow and ebb, not twice, as in other ports, but once only every day, and the flood happens at the setting and the greatest ebb at the rising of the Moon. This tide increases with the declination of the Moon till the 7th or 8th day; then for the 7 or 8 days following, it decreases at the same rate as it had increased before, and ceases when the Moon changes its declination, crossing over the equator to the south. After which the flood is immediately changed into an ebb; and thenceforth the ebb happens at the setting, and the flood at the rising of the Moon; till the Moon again passing the equator changes its declination. There are two inlets to this port, and the neighbouring channels, one from the seas of *China*, between the continent and the island of *Luconia*, and the other from the *Indian* sea, between the continent and the island of *Borneo*. But whether there be really two tides propagated through the said channels, one from the *Indian* sea in the space of 12 hours, and one from the sea of *China* in the space of 6 hours, which therefore happening at the 3rd and 9th lunar hours, by being compounded together, produce those motions, or whether there be any other circumstances in the state of those seas, I leave to be determined by observations on the neighbouring shores."

It is impossible not to contrast the soundness of this remarkable paragraph with the absurdity of the popular explanation of the double high water at Southampton, more than two centuries later.

Newton concludes the proposition as follows: "Thus I have explained the causes of the motions of the . . . Sea. Now it is fit to subjoin something concerning the quantity of those motions."

BOOK III., PROPOSITION 36.

The object of this proposition is "to find the force of the Sun to move the Sea." The component of force-intensity along *PT* is

$$\gamma S \frac{a}{d^3} = \frac{S}{T} \frac{a^3}{d^3} g,$$

where g is the ordinary gravitational intensity at the Earth's surface, calculated as if all the Earth's mass were concentrated in its centre, by Book I., Proposition 76. Here, of course, P is a point on the Earth's surface, but if we take P to be the Moon, we have, from Book I., Proposition 66, Corollary 17,

$$\frac{S}{T} \frac{a^3}{d^3} = \left(\frac{27 \text{ d. } 7 \text{ h. } 43 \text{ m.}}{365 \text{ d. } 6 \text{ h. } 9 \text{ m.}}\right)^2 = \frac{1000}{178725}.$$

Assuming the distance of the Moon to be 60 times the radius of the Earth, the required force-intensity along *PT* is seen to be

$$\frac{1}{(60)^3} \times \frac{1000}{178725} g \quad \text{or} \quad \frac{g}{38604600}.$$

In the *Corollary* to this proposition Newton calculates, on the equilibrium-hypothesis, the difference between the greatest and least heights of water. By the equilibrium-hypothesis we mean that the water is supposed to lose its inertia without losing its gravitational properties. In Book III., Proposition 19, he had calculated the difference between the equatorial and polar radii of a rotating homogeneous liquid Earth and had found it to be 85472 Paris feet. In doing so he had utilised the artifice of imagining two wells cut from the surface to the centre of the Earth, one along the axis of rotation and the other at right angles to this axis. In such wells, of course, the forces are *vertical*. Let us now consider an ocean completely covering a solid sphere of equal density, and suppose that the whole is rotating about an axis with angular speed ω, and subject to its own gravitation only. The deformation of the ocean surface will be the same as that of a homogeneous liquid Earth, but may be regarded as produced by the *horizontal* component of a force-intensity $\omega^2 x$ (Fig. 3), where CD is now the Earth's axis and TS is any perpendicular direction. The distribution of this horizontal component with respect to CD is exactly similar to that of the tide-generating force with respect to TS, the ratio of their intensities being

$$\omega^2 : -3\gamma S/d^3.$$

This ratio may be written

$$\frac{\omega^2 a}{g} : -\frac{3}{g}\,(\text{force-intensity in } PT)$$

or

$$\frac{1}{289} : -\frac{3}{38604600}.$$

It follows that on the equilibrium-hypothesis the difference between the greatest and least heights of water is

$$\frac{3 \times 289 \times 85472}{38604600} \text{ Paris feet}$$

or 23 Paris inches. The reference to horizontal components in the above argument is not taken from Newton, but has been given to emphasise the validity of his process.

Book III., Proposition 37.

The object of this proposition is "to find the force of the Moon to move the Sea." Newton begins: "The force of the Moon to move the Sea is to be deduced from its proportion to the force of the Sun, and this proportion is to be collected from the proportion of the motions of the Sea, which are the effects of those forces." In this task there are many pitfalls, and in fact Newton's estimate is much too large.

In *Corollary* 1 he indicates a reason for the small tides at oceanic islands and for the increased tides and tidal currents in shallow bays and channels.

THE SYSTEM OF THE WORLD.

In this work there is an account of the theory of the tides, much of which appears to be a draft of parts of the treatment in Book III. of the *Principia.*

In discussing the relative magnitudes of tides on the shores and in the middle of oceans Newton indicates what is now called the "correction to the equilibrium-hypothesis for the presence of continents." According to this and in a general way, the magnitude of the tides of an ocean increases with the size of the ocean.

His calculation of the difference between the greatest and least heights of water in the solar equilibrium-tide differs somewhat from that in the *Principia.* The artifice of the two wells is here applied directly to the tides, one being along the line TS and the other at right-angles to TS. Now in Book I., Proposition 73, Newton had proved that the intensity of gravitation inside a homogeneous sphere is proportional to the distance from the centre of the sphere. So also are the force-intensities of Fig. 3, when these act on the water in the two wells. The resultant force-intensities at the same distance from the Earth's centre in the two wells are therefore in the ratio

$$1 - \frac{2}{38604600} : 1 + \frac{1}{38604600}$$

or

$$1 : 1 + \frac{1}{12868200}$$

approximately. Now the pressure-intensities at the bottoms of the wells will be proportional to these force-intensities and to the squares of the heights. But the pressure-intensities at the bottoms must be equal, and therefore the heights will be inversely proportional to the square roots of the force-intensities, *i.e.* they will be in the ratio

$$1+\frac{1}{25736400}:1$$

approximately. On taking the radius of the Earth to be 19615800 Paris feet, the difference in the heights of the two wells is

$$\frac{19615800}{25736400}\text{ Paris feet}$$

or 9 Paris inches. It will be noticed that in this calculation Newton neglects the attraction of the disturbed ocean, which he had taken into account in Book III., Proposition 19. This fact entirely accounts for the difference between the two estimates of 23 and 9 inches.

The chief specific references of Newton to the theory of the tides have now been mentioned, but much of the *Principia* is really an indirect contribution to the subject. For a complete theory of the tides requires a complete theory of the motions of the Earth, Moon and Sun.

NEWTON'S CONTRIBUTION TO THE GEOMETRY OF CONICS.

By the Rev. J. J. Milne, M.A.

In an article dealing with the conics of Newton we must always keep in mind the object with which they were written. When the law had been established that the orbit of a planet is a conic section described under the action of a force situated at the focus, two classes of problems at once presented themselves.

(1) *To describe an orbit having given the focus and three other conditions.*

(2) *To describe an orbit satisfying five conditions, the focus not being given.*

The determination of these orbits depends upon properties, some of which had not been previously considered by writers on conics, and these Newton discovered and demonstrated and are what he termed Lemmas. The first we meet with is in Bk. I. Sect. iii. Lemma xiv.

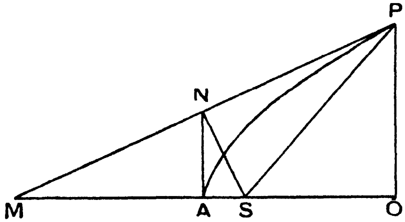


Fig. 1.

The perpendicular SN let fall from the focus S of a parabola on its tangent at any point P is a mean proportional between the distances of the focus from the point of contact, and from the principal vertex A.

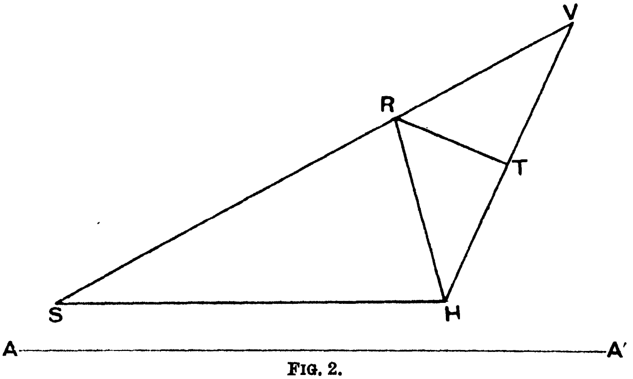


Fig. 2.

Cor. 1. $PS^2 : SN^2$ *as* $PS : SA$.

Cor. 2. *Because* SA *is given,* SN^2 *will be as* PS.

Cor. 3. *The point of intersection of any tangent with the perpendicular on it from the focus lies in the tangent at the principal vertex.*

These properties and the following Lemma are now given in all text books on conics, and the proofs need not be repeated here.

Bk. I. Section iv. Lemma xv. *S, H being the foci of a conic, AA', the length of the major axis, and V any point on a circle, centre S, and radius equal to AA', HV is joined and bisected at right angles in T by the line TR meeting SV in R. Then TR is a tangent to the conic at the point R.*

SECTION IV.

Of the finding of elliptic, parabolic and hyperbolic orbits, having given the focus.

Prop. xviii. To describe a central conic given the focus, the length of the major axis and (1) *one point and one tangent,* (2) *two points,* (3) *two tangents.*

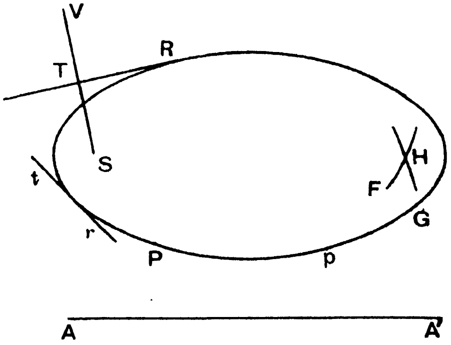


FIG. 3.

Let S be the focus, AA' the length of the major axis, P a given point on the orbit and TR a given tangent. With centre P and radius $AA' \pm SP$ describe a circle HG. On the tangent TR let fall the perpendicular ST and produce it to V so that $TV = ST$, and about V as centre and AA' radius describe the circle FH. In this manner, whether two points P, p are given, or two tangents TR, tr, or a point P and a tangent TR, we are to describe two circles. Let H be their common intersection, and from the foci S, H, with the given axis describe the trajectory. I say the thing is done. For because $PH + SP$ in the ellipse, and $PH - SP$ in the hyperbola, is equal to the axis, the described trajectory will pass through the point P, and by Lemma xv. will touch the straight line TR. And by the same argument it will either pass through the two points P, p or touch the two straight lines TR, tr.

Prop. xix. To describe a parabolic trajectory with given focus which shall pass through given points and touch given straight lines.

Let S be the focus, P a point and TR a tangent of the trajectory to be described. With P as centre and radius PS describe a circle FG. From S draw ST perpendicular to TR, and produce it to V so that $TV = ST$. In the same way another circle fg is to be described if another point p is given; or another point v is to be found if another tangent tr is given. Then draw the straight line IF which shall touch the two circles FG, fg if two points P, p are given; or pass through the two points V, v, if two tangents TR, tr

are given; or touch the circle *FG* and pass through the point *V*, if the point *P* and the tangent *TR* are given. On *FI* let fall the perpendicular *SI* and bisect *SI* in *K*; and with the axis *SK* and principal vertex *K* describe a parabola; I say the thing is done. For this parabola (because $SK = IK$ and $SP = FP$) will pass through the point *P*; and (by Cor. 3, Lemma xiv.) because *ST* is equal to *TV*, and *STR* a right angle, it will touch the straight line *TR*.

Prop. xx. About a given focus to describe any trajectory given in species which shall pass through given points, and touch given straight lines.

In this proposition I only give Newton's construction. The proof can readily be supplied by the reader.

Case 1. *Given two points B, C.*

Because the trajectory is given in species, the ratio of the principal axis to the distance between the foci will be given. In that ratio take *KB* to *BS*, and *LC* to *CS*. With centres *B*, *C* and radii *BK*, *CL* describe two circles; and on the straight line *KL* which touches the same in *K* and *L*, let fall the perpendicular *SG*. Divide *SG* internally at *A* and externally at *a* so that *GA* may be to *AS*, and *Ga* to *aS* in the ratio *KB* to *BS*. Then with the axis *Aa* and focus *S* describe the trajectory.

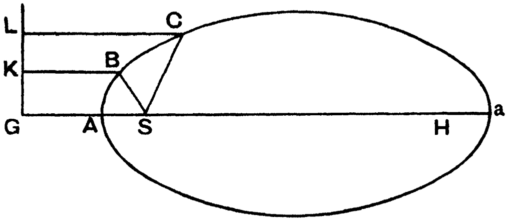


FIG. 4.

Case 2. *Given two tangents TR, tr.*

From the focus on those tangents let fall the perpendiculars *ST*, *St*, which produce to *V*, *v* so that *TV*, *tv* may be equal to *TS*, *tS*. Bisect *Vv* in *O* by the perpendicular *OH*, and divide *VS* internally in *K* and externally in *k* so that *VK* may be to *KS* and *Vk* to *kS* as the principal axis is to the distance between the foci. On the diameter *Kk* describe a circle cutting *OH* in *H*, and with the foci *S*, *H* and principal axis equal to *VH* describe the trajectory.

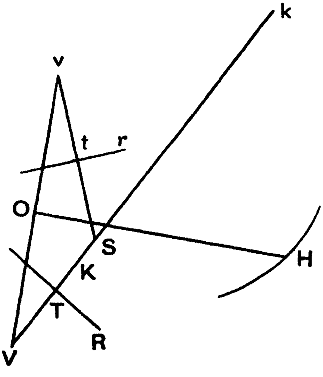


FIG. 5.

Case 3. *Given a tangent TR and its point of contact R.*

On TR let fall the perpendicular ST and produce it to V so that TV may be equal to ST. Join VR and divide VS internally at K and externally at k so that VK may be to SK and Vk to Sk as the principal axis to the distance between the foci. On Kk as diameter describe a circle cutting VR produced in H. Then with foci S, H and major axis equal to VH describe the trajectory.

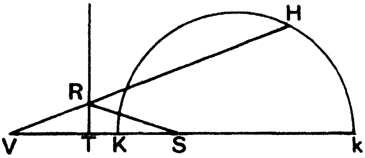


FIG. 6.

Case 4. *Given a tangent TR and a point P not on TR.*

Draw ST perpendicular to TR and produce it to V so that $TV = ST$. Join SP, PV. Take a line ab of any given length. On ab as major axis with the given eccentricity describe a conic. Let s, h be its foci. Make the angle $shq = SVP$ and $hsq = VSP$, so that the triangle shq is similar to the triangle SVP. With centre q and radius which shall be to ab as SP to SV describe a circle cutting the conic in p. Join sp. Make the angle $PSH = psh$, and on the line SH take a point H such that SH is to sh as SP to sp. Then the conic constructed with S, H as foci is the trajectory required.

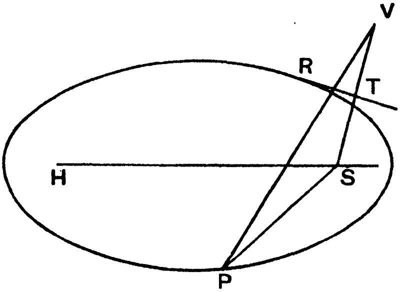


FIG. 7.

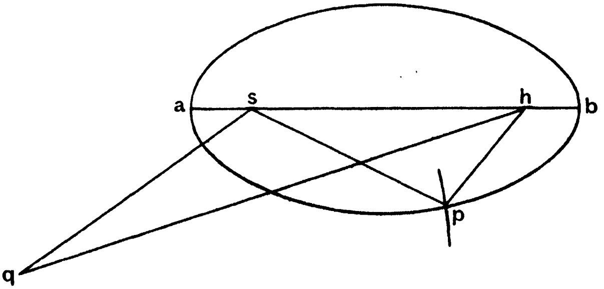


FIG. 8.

Lemma xvi. From three given points to draw to a fourth point that is not given three right lines whose differences shall be given, or none at all.

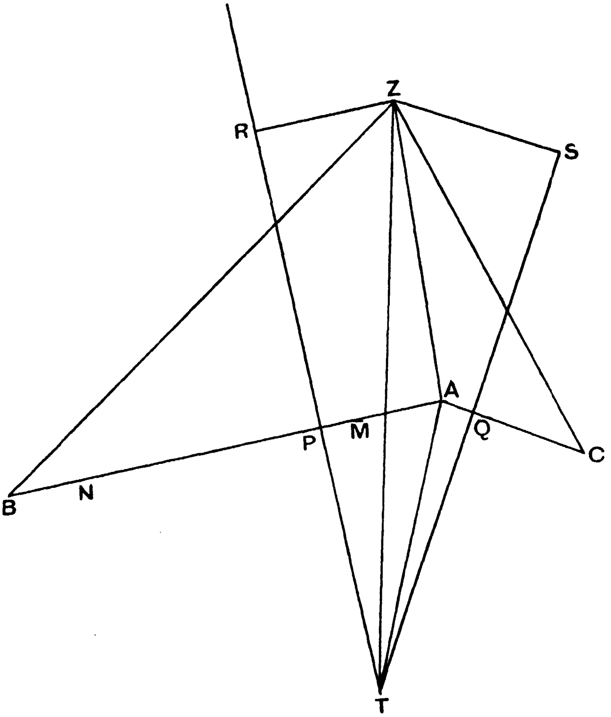


FIG. 9.

Case 1. Let the given points be A, B, C, and Z the fourth point which we are to find. Because of the given difference of the lines AZ, BZ, the locus of the point Z will be an hyperbola whose foci are A, B, and whose principal axis is the given difference. Let that axis be MN, and on it, taking PM to MA as MN is to AB, erect PR perpendicular to AB, and let fall ZR perpendicular to PR. Then from the nature of the hyperbola ZR will be to AZ as MN is to AB. Similarly the locus of the point Z will be another hyperbola, whose foci are A, C, and whose principal axis is the difference between AZ and CZ; and as before, QS a perpendicular on AC may be drawn to which (QS) if from any point Z of this hyperbola a perpendicular ZS is let fall, this ZS shall be to AZ as the difference between AZ and CZ is to AC. Wherefore the ratios of ZR and ZS to AZ are given, and consequently the ratio of ZR to ZS. Therefore if RP and SQ meet in T, and TA, TZ be drawn, the figure $TRZS$ will be given in species, and the line TZ, in which the point Z is somewhere placed, will be given in position. There will be given also the right line TA, and the angle ATZ; and because the ratios of AZ and TZ to ZS are given, their ratio to each other is given also; and thence will be given likewise the triangle ATZ, whose vertex is the required point Z.

Case 2. If two of the three lines, *e.g.* AZ and BZ, are equal, draw TZ so as to bisect AB at right angles coinciding with TR. Then find the triangle ATZ as above.

Case 3. If all the three are equal, the point Z will be the centre of the circle passing through the points A, B, C.

The problem of this Lemma was solved by Vieta also, in his restoration of Apollonius' last book *On Contacts.*

Prop. xxi. About a given focus to describe a trajectory that shall pass through given points and touch right lines given in position.

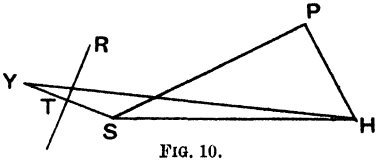


FIG. 10.

Let the focus S, the point P and the tangent TR be given, and suppose the other focus H is to be found. Draw ST perpendicular to TR and produce to Y so that $TY=ST$. Then by Lemma xv., YH is equal to the principal axis. Join SP, HP. Then SP is the difference between HP and the principal axis. Similarly if more tangents TR are given, or more points P, we can always determine as many lines YH or PH, drawn from the said points Y or P to the focus H, which either shall be equal to the axis or differ from the axis by given lengths SP; and therefore which shall either be equal among themselves or shall have given differences, from whence, by the preceding Lemma, the other focus is given. But having the foci and the length of the axis (which is either YH, or, if the trajectory be an ellipse, $PH+SP$; or $PH-SP$, if it be an hyperbola) the trajectory is given.

SCHOLIUM.

The case where three points are given is more readily solved thus.

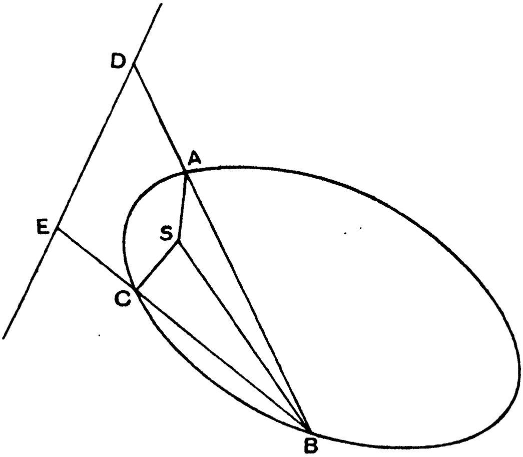


FIG. 11.

Let A, B, C be the three points, S the focus. Join BA and produce it to D so that $SA : SB$ as AD to BD. Join BC and produce it to E so that $SC : SB$ as CE to BE. Then DE will be the directrix, and the trajectory can be described.

A similar solution is given in De la Hire's *Conics*, Bk. viii. Prop. xxv.

SECTION V.

How the orbits are to be found when neither focus is given.

We now come to a different class of problems. These are preceded by five Lemmas, xvii.-xxi., which are quite of an elementary nature, and depend chiefly upon the property that if two chords of a conic intersect, the rectangles contained by their segments are proportional to the squares on the parallel diameters, and, of course, this ratio is unchanged when the chords are moved parallel to themselves. This is sometimes called Newton's Theorem, but Newton expressly refers it to Apollonius, Bk. III. Props. 17-23, and it is, without doubt, the property to which Apollonius refers in his general preface as having been discovered by himself, and by means of which he was enabled to obtain a complete solution of the *locus ad tres et quatuor lineas.*

These with the properties of similar triangles and quadrilaterals as treated in the 6th book of Euclid are all that are required in the consideration of these Lemmas, yet in spite of their elementary nature and extremely interesting results and novel method of treatment, it is very seldom that any notice is taken of them in the text books of the present day, and for this reason I propose to consider them very fully.

Lemma xvii. If ABDC is a quadrilateral inscribed in a conic, and from any point P on the curve straight lines PQ, PR, PS, PT are drawn making given angles with the sides AB, CD, AC, BD, then will PQ . PR be to PS . PT in a constant ratio.

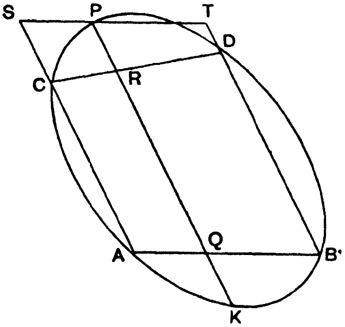


FIG. 12.

There are three cases.

In Case 1 the sides *AC, BD* are parallel, and through *P* is drawn a line *PRQ* parallel to them, cutting *AB* in *Q* and *CD* in *R*, and another line *SPT* parallel to *AB*, cutting *AC* in *S* and *BD* in *T*. Then producing *PQ* to *K*, making $QK=PR$, the point *K* is on the conic; and by Ap. III. Props. 17, 19, 21, $PQ \cdot QK : AQ \cdot QB$, *i.e.* $PQ \cdot PR : PS \cdot PT$ in the ratio of the squares of the parallel diameters.

In Case 2 *BD* is no longer parallel to *AC*, but *PRQ* and *SPT* are still parallel respectively to *AC* and *AB*.

A line is drawn through *B* parallel to *AC* meeting the conic in *d* and the line *SPT* in *t*. *Cd* is joined, cutting *PRQ* in *r*, and through *D* is drawn a line parallel to *PRQ* cutting *Cd* in *M* and *AB* in *N*.

Then from similar triangles:

$$DN : NB = Bt \text{ (or } PQ) : Tt,$$

$$DM : AN = \quad Rr : AQ \text{ (or } PS);$$

$\therefore\ DN\,.\,DM : AN\,.\,NB = PQ\,.\,Rr : PS\,.\,Tt,$

$= PQ\,.\,Pr : PS\,.\,Pt,$ ....by Case 1,

$= PQ\,.\,PR : PS\,.\,PT.$

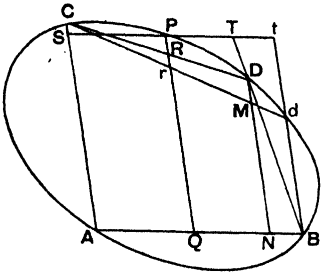


FIG. 13.

In Case 3 the lines PQ, PR, PS, PT are no longer parallel to the sides AC, AB, but now make given angles with the four sides of the quadrilateral $ABDC$. Through P are drawn two lines Prq, sPt parallel respectively to AC, AB.

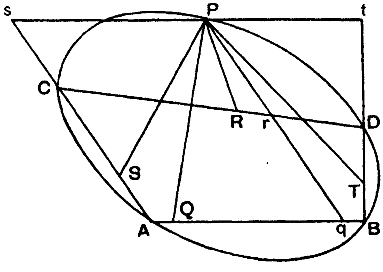


FIG. 14.

Then, because the angles of the triangles PQq, PRr, PSs, PTt are given, the ratios $PQ : Pq$; $PR : Pr$; $PS : Ps$; $PT : Pt$ will be also given, and $\therefore$ the ratios $PQ\,.\,PR : Pq\,.\,Pr$; and $PS\,.\,PT : Ps\,.\,Pt$ will be also given. And by Case 1, the ratio $Pq\,.\,Pr : Ps\,.\,Pt$ is given; $\therefore$ so also is the ratio

$$PQ\,.\,PR : PS\,.\,PT.$$

Lemma xviii. is the converse of xvii., and proves that:

If the point P moves in such a manner that $PQ\,.\,PR : PS\,.\,PT$ in a given ratio, the locus of P is a conic passing through the points A, B, D, C.

Take p to be any position of the moving point, and suppose a conic to be described through the five points A, B, C, D, p. Then the point P will always lie on this conic. For if not, join AP cutting the conic in b. Through the points p, b draw the lines pq, pr, ps, pt and bk, bn, bf, bd making the given angles with the sides of the figure $ABDC$.

Then by Lemma xvii., $bk\,.\,bn : bf\,.\,bd = pq\,.\,pr : ps\,.\,pt$

$= PQ\,.\,PR : PS\,.\,PT$, by supposition,

and because of the similarity of the figures $bkAf$, $PQAS$,

$$bk : bf = PQ : PS;$$

$\therefore$ by division, $\quad bn : bd = PR : PT.$

∴ the equiangular figures *Dnbd*, *DRPT* are similar, and consequently their diagonals *Db*, *DP* coincide. Wherefore *b* falls in the intersection of *AP* and *DP*, and ∴ coincides with *P*. ∴ the point *P*, wherever it is taken, lies on the assigned conic section.

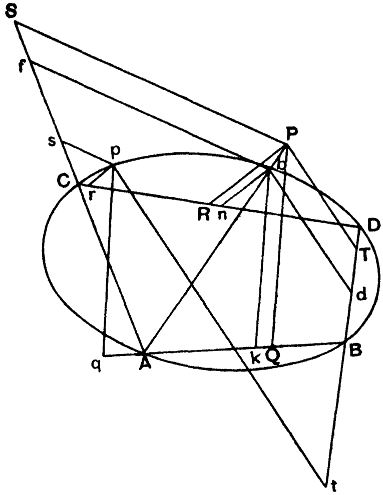


FIG. 15.

In the corollary it is shewn that if *BD* approaches to, and coincides with *AC*, and the line *PT* coincides with *PS*, and the rectangle $PQ\,.\,PR : PS^2$ in a given ratio, the locus of *P* is a conic touching *AB*, *CD* in *A* and *C* ; and the contrary.

SCHOLIUM.

It is stated that the name of conic section is to be understood as including a circle and two straight lines. Also that the figure *ABDC* may be a quadrilateral whose two opposite sides cross one another like diagonals. Also, one or two of the four points *A*, *B*, *C*, *D* may be supposed removed to infinity.

Lemma xix. To find a point P from which if four lines PQ, PR, PS, PT are drawn to as many other given straight lines AB, CD, AC, BD at given angles, the rectangle PQ . PR shall be to PS . PT in a given ratio.

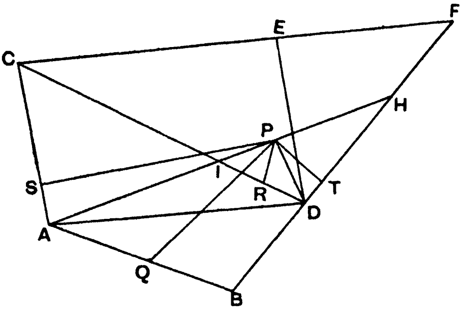


FIG. 16.

From Lemma xviii. we know that the locus of P is a conic circumscribing $ABCD$. Through A draw any straight line meeting BD in H and CD in I. Let it be required to find the point where the line AIH cuts the conic. Let us assume that P is the required point. Then since all the angles of the figure are given, the ratios of $PQ : PA$ and $PA : PS$, and $\therefore PQ : PS$ is given.

And since the ratio $PQ . PR : PS . PT$ is given, $\therefore PR : PT$ is given.

Also the ratios $PI : PR$ and $PT : PH$ are given, the ratio $PI : PH$ is known, and $\therefore$ the position of P is known.

Cor. 1. Suppose it required to draw a tangent to the conic at any point D. When the point P approaches indefinitely near to D, the chord PD becomes a tangent.

When the line $AIPH$ rotates about A to the position AD, the ratio $IP : PH$ remains unchanged. Therefore if we draw CF parallel to AD, and cut it in E in the same ultimate ratio, DE will be the tangent, because CF and the evanescent IH are parallel, and are similarly cut in E and P.

Cor. 2. Through any of the points A, B, C, D as A draw the tangent AE by Cor. 1, and through B draw a line BF parallel to AE, and by the Lemma find the point F where BF cuts the conic. Bisect BF in G. Then AG produced is the diameter to which BG is ordinate. Let AG meet the locus in H. Then AH is a diameter, and if L is its latus rectum,

$$L : AH = BG^2 : AG . GH.$$

If AG nowhere meets the locus, the line AH being infinite the locus will be a parabola, and its latus rectum corresponding to the diameter AG will be $\frac{BG^2}{AG}$.

But if it does meet it anywhere, the locus will be a hyperbola when the points A and H are on the same side of the point G; and an ellipse if the point G falls between A and H; except when the angle AGB is a right angle, and at the same time BG^2 is equal to the rectangle $AG. GH$, in which case the locus will be a circle.

Newton concludes: "And so we have given a solution of that famous problem of the ancients concerning four lines, begun by Euclid, and carried on by Apollonius; and this not an analytical calculus, but a geometrical composition, such as the ancients required."

THE LOCUS *AD TRES ET QUATUOR LINEAS.*

At this point an account of this locus may not be considered out of place, as there is no problem in geometry which surpasses it in interest and in its power and far-reaching consequences.

Apollonius, in the general preface to his *Conics*, says that the locus was incompletely solved by Euclid, but that by means of properties which he himself had discovered and given in his third book it was possible to give a complete demonstration of it. He makes no further mention of it, but the properties referred to were no doubt those in Bk. III. (16-23). Pappus (*Hultsch*, p. 678) speaks rather disparagingly about Apollonius, calling him a boaster, but does not doubt that he obtained a solution, hinting that he had obtained it owing to his having been intimate for a long time with the pupils of Euclid at Alexandria. Eutocius could make nothing of it.

Descartes (1633), after spending five or six weeks on it (*Cousin*, vol. vi. pp. 224, 294), at last obtained a solution by means of algebraical geometry, and he made the problem the basis of his work *La Géométrie*, in which it occupies a considerable part of the first two books. After shewing that in every case the equation of the locus is of the second degree, he concludes (p. 334): " Au reste à cause que les equations, qui ne montent que jusques au quarré, sont

toutes comprises en ce que je viens d'expliquer, non seulement le problèsme des anciens en 3 et 4 lignes est icy entièrement achevé; mais aussy tout ce qui appartient à ce qu'ils nommoient la composition des lieux solides."

As we have seen, Newton was successful in obtaining a solution, and he concludes it with the words: "Atque ita problematis veterum de quatuor lineis ab Euclide incæpti et ab Apollonio continuati non calculus, sed compositio geometrica, qualem veteres quaerebant, in hoc corollario exhibetur," in which he has a sly hit at the superiority of the *compositio geometrica* over the calculus.

In the spring of 1894 my friend R. F. Davis and I brought out through Macmillans a work on *Geometrical Conics,* and as I had always taken an interest in Greek Geometry I made a point of indicating in a footnote every property given in our book which was to be found in Apollonius. When I came to consider the *locus ad tres et quatuor lineas,* without which our book would not have been complete, I was obliged to follow the lead of all previous geometricians, and say that: "The solution which Apollonius tells us in his general preface was contained in his third book has unfortunately not been handed down to us" (cf. Taylor's *Geometry of Conics*, pp. lxvi, 266). Newton's proof, of course, I was able to give, but, as will be noticed, he first proves it for the inscribed quadrilateral, and then deduces the corresponding property for the triangle, and I thought it would be better, if possible, to prove it first for the triangle, and then deduce it for the quadrilateral, as it is quoted in this order by both Apollonius and Pappus (Bk. VII. c. 36).

After a considerable amount of thought extending over several months, I succeeded in doing this, and there I left it with the remark that "It is not improbable that Apollonius proved the two theorems in the order in which he refers to them."

I may add that at the end of Ap. Bk. III. there are three propositions (54, 55, 56), but they always seemed to me very complicated, and to lead to results which I could not interpret, and I thought it best to leave them alone.

In the autumn of the same year 1894, E. M. Langley, who was the editor of the *Gazette,* asked me if I would read a paper at the Christmas meeting, and as my mind had been so recently occupied with the study of Apollonius I promised to read one on that author, the line I took being to bring out the properties which were known to Apollonius, and to indicate those which had been discovered since his time. As no English book specially dealt with Apollonius, Sir T. L. Heath's book (1896) not having yet appeared, it seemed to me that it would make an interesting paper if I were to summarise the contents of the different groups of propositions in each book. Of course this brought me up against Props. 54, 55, 56 of Book III., of which I felt I must give some account; but though I struggled through them several times, I did not feel able to say what was their object until the idea suggested itself to me that possibly they were the sepulchre in which the missing solution of the *locus* was entombed. Exploration of this avenue, as the politicians say, soon revealed the *locus ad tres lineas* in its beautiful simplicity.

In order that the reader may be able to understand the position in which Apollonius left the locus, I give the enunciation of Prop. 54 of Bk. III.

If two straight lines touching the section of a cone or the circumference of a circle intersect one another, and through the points of contact parallels are drawn to the tangents, and from the points of contact straight lines are drawn to the same point of the curve cutting the parallels, the rectangle contained by the intercepted parts has to the square on the line joining the points of contact the ratio compounded of that which the square on the interior part of the straight line joining the point of intersection of the tangents and the middle point of the straight line joining the points of contact has to the square on the remainder and of that which *the rectangle contained by the tangents has to the fourth part of the square on the line joining the points of contact.* i.e. *in Fig.* 17:

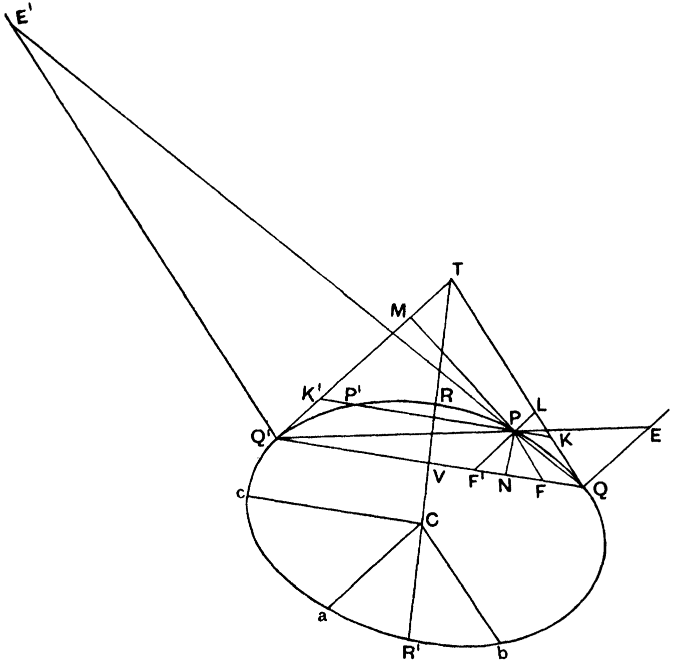


FIG. 17.

TQ, TQ' are two given tangents to a conic, and $Q'E'$, QE straight lines parallel to them. Through any point P on the curve are drawn QP, $Q'P$ meeting $Q'E'$ and QE in E' and E. $TRVR'$ is the diameter through T; then

$$QE \, . \, Q'E' : QQ'^2 = (RV^2 : TR^2) \times (TQ \, . \, TQ' : \tfrac{1}{4}QQ'^2). \, \ldots\ldots\ldots\ldots (1)$$

Props. 55, 56 are the corresponding propositions for the hyperbola in the cases in which the tangents are drawn (1) to the two opposite branches, (2) to the same branch of the curve.

[It may be mentioned that the above result can easily be reduced to the form $QE \, . \, Q'E' = \text{const.}$, but I wish to be brief, and I leave the proof to the reader.]

Assuming Apollonius' proof of (1), now draw PL, PM, PN perpendiculars on the sides of the triangle TQQ', and draw PF, PF' parallel to TQ and TQ', and let Ca, Cb, Cc be the semi-diameters parallel to the given lines QE, $Q'E'$, QQ' respectively. Then noticing that $RV : TR = R'V : R'T$, since $TRVR'$ is a harmonic range, we have from the similar triangles EQQ', $PF'Q'$,

$$QE : QQ' = PF' : F'Q' \text{ (or } PK') = PN : PM,$$

$$Q'E' : QQ' = PF : FQ \text{ (or } PK) = PN : PL;$$

$$\begin{aligned} \therefore \; PN^2 : PL \, . \, PM &= QE \, . \, Q'E' : QQ'^2, \\ &= (RV \, . \, VR' : TR \, . \, TR') \times (TQ \, . \, TQ' : QV \, . \, VQ'), \\ &= (RV \, . \, VR' : QV \, . \, VQ') \times (TQ \, . \, TQ' : TR \, . \, TR'), \\ &= (CR^2 : Cc^2) \times (Ca \, . \, Cb : CR^2), \\ &= Ca \, . \, Cb : Cc^2, \end{aligned}$$

which is of the form $\alpha\beta = K\gamma^2$, where $K = \dfrac{Cc^2}{Ca \, . \, Cb}$.

From this we can easily deduce the *locus ad quatuor lineas* in the form $\alpha\beta = K\gamma\delta$, in which $K = \frac{Cc \,.\, Cd}{Ca \,.\, Cb}$, where Ca, Cb, Cc, Cd are the semi-diameters parallel to the sides of the quadrilateral to which α, β, γ, δ are respectively perpendicular.

My explanation of the matter is as follows:

Apollonius worked out for himself a complete solution of the locus on the above lines. He then deliberately gave only half the proof as a separate proposition, and left it to his readers to discover for themselves what it was about, and to finish the proof for themselves. It was intended to be a mathematical puzzle or challenge, and is quite in accordance with Pappus' remark that Apollonius left a great many things in his conics incomplete. I think the most extraordinary thing about it is that it should have taken 2000 years to guess the riddle.

Before leaving this locus, which in point of interest can compare with any of the problems known to the ancients, the history of which makes the study of mathematics such a fascinating subject, I would like to add a word respecting its power. In its form $QE \,.\, Q'E' = \text{const.}$ it gives us the anharmonic property of points on a conic, viz. *Given two homographic pencils not in perspective, the intersections of corresponding rays lie on a conic which passes through the centres of the pencils,* which again gives us the theorems of Pascal and Desargues, the *organica descriptio* of Newton and Maclaurin, and leads directly to the description of a conic satisfying five conditions, and is, in brief, the *fons et origo* of all these important propositions.

Lemma xx. If the two opposite angular points A and P of any parallelogram ASPQ lie on a conic, and the sides AQ, AS meet the conic in B and C, and to any fifth point D on the conic are drawn BD and CD meeting the other two sides PS, PQ of the parallelogram in T and R, PR and PT will be one to another in a given ratio. Conversely, if PR and PT are in a given ratio, the locus of the point D will be a conic passing through A, B, C, P.

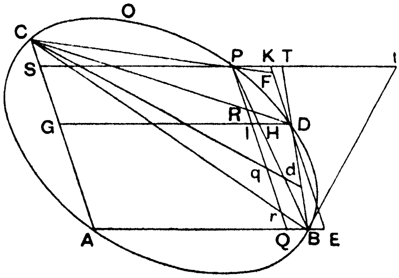


FIG. 18.

Join BP, CP, and through D draw DG parallel to AB, meeting PB, PQ, CA in H, I, G; and draw DE parallel to AC meeting PC, PS, AB in F, K, E. Then by Lemma xvii.,

$$DE \,.\, DF : DG \,.\, DH \text{ in a given ratio.}$$

But
$$PQ : DE \text{ (or } IQ) = PB : HB,$$
$$= PT : DH;$$
$$\therefore\ PQ : PT = DE : DH.$$

Similarly
$$PR : DF = RC : DC,$$
$$= (IG \text{ or}) \; PS : DG;$$
$$\therefore \; PR : PS = DF : DG;$$
$$\therefore \; PQ \,.\, PR : PS \,.\, PT = DE \,.\, DF : DG \,.\, DH,$$
$$= \text{a given ratio.}$$

But PQ and PS are given; ∴ the ratio $PR : PT$ is given. Q.E.D.

Conversely, if the ratio $PR : PT$ is given, then by going back again, by a like reasoning it will follow that the rectangle $DE \,.\, DF : DG \,.\, DH$ in a given ratio, and ∴ by Lemma xviii. the point D will lie in a conic passing through A, B, C, P.

Cor. 1. Hence if we draw BC cutting PQ in r, and in PT take
$$Pt : Pr = PT : PR,$$
then Bt will be the tangent at B. For suppose the point D to coalesce with the point B, so that the chord BD vanishes and BT becomes a tangent, CD and BT will coincide with CB and Bt.

Cor. 2. Conversely, if Bt is a tangent, and the lines BD, CD meet in any point D of a conic, $PR : PT = Pr : Pt$. And if $PR : PT = Pr : Pt$, then BT and CR will meet in some point D on a conic.

Cor. 3. One conic cannot cut another conic in more than four points. For if possible let two conics pass through the five points A, B, C, P, O, and let the straight line BD cut them in D, d.

Join Cd cutting PQ in q. Then $PR : PT = Pq : PT$; ∴ $PR = Pq$, contrary to supposition.

DESCRIPTIO ORGANICA.

Lemma xxi. Two angles DBM, DCM of given magnitudes rotate about their vertices B, C, which are fixed. If the intersection of two of their sides BM, CM move along a fixed straight line MN, the intersection D of the other two sides BD, CD will describe a conic passing through B, C. And conversely, if the straight lines BD, CD at their point of intersection describe a conic passing through the points B, C, and the angles DBM, DCM are of given magnitudes, the point M will describe a straight line.

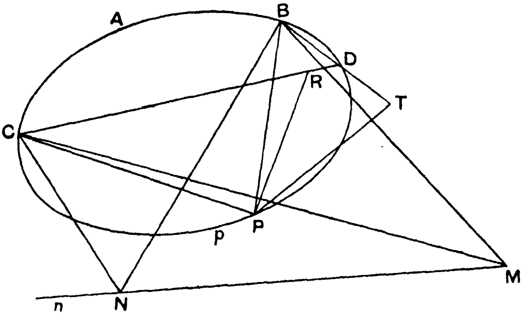


FIG. 19.

Suppose that when the variable point M is at a given point N in the line MN, the variable point D is in the position P. Join BN, CN, BP, CP, and at P make the angle $BPT = BNM$ and $CPR = CNM$.

Then since
$$MBD = NBP \quad \text{and} \quad MCD = NCP,$$
$$\therefore \; MBN = PBT \quad \text{and} \quad NCM = PCR;$$

∴ the triangles NBM and PBT are similar, as are also NCM, PCR;
$$\therefore \; PT : MN = PB : NB \quad \text{and} \quad PR : MN = PC : NC.$$

But the points *B*, *C*, *N*, *P* are given; ∴ *PT* and *PR* have a given ratio to *MN*, and consequently a given ratio between themselves. And *PT* and *PR* are drawn in fixed directions; ∴ by Lemma xx. the point *D*, where the moveable lines *BT* and *CR* intersect, is a conic passing through *B*, *C*, *P*.

To prove the converse, suppose that when the moveable point *D* falls successively on the two points *p*, *P* of the conic, the moveable point *M* falls on the points *n*, *N*. Then the moveable point *M* will lie in the straight line *nN*. For if possible let the point *M* describe any curve line. Then the point *D* will describe a conic passing through the five points *A*, *B*, *C*, *P*, *p*, whilst *M* describes a curve line passing through the points *N*, *n*. But by the first part, the point *D* will also describe a conic through the same five points when the point *M* describes the straight line *MNn*. Therefore the two conics will both pass through the same five points, which is contrary to Lemma xx. Cor. 3. It is therefore absurd to suppose that *M* describes a curve line.

Having established these Lemmas, Newton proceeds to employ them to describe a conic subject to five conditions.

The first case which he takes is that where five points are given. We meet with a solution of this problem in Pappus, VIII. Props. 13, 14, where the method depends on Apollonius, III. Props. 16-23. Newton gives two solutions, one employing Lemma xx., and the other by means of his *descriptio organica*, both of which really depend on the same propositions of Apollonius. Other solutions can be obtained by using the anharmonic property of points on a conic, or the theorems of Maclaurin, Pascal or Desargues, and the principle common to all these is the *locus ad tres et quatuor lineas*, and now we see why Newton attacked this problem. It presented itself naturally to him on his way to the *inventio orbium*.

He was now able to construct a conic having given

I. Five points.

II. (1) Four points and one tangent, (2) four tangents and one point.

III. (1) Three points and two tangents, (2) three tangents and two points.

As these constructions are now given in most text books on the subject, and can be dealt with more simply by the methods of cross-ratio geometry, I need not reproduce them here.

It may be mentioned that to describe a conic having given (1) two points and three tangents, and (2) one point and four tangents, Newton makes use of the semi-analytical method of transformation given in the following Lemma:

Lemma xxii. To transform figures into other figures of the same kind.

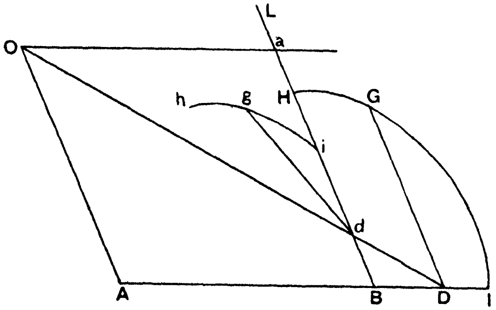


FIG. 20.

Suppose that any figure *HGI* is to be transformed. Draw any two parallel lines *AO*, *BL* cutting any given line *AB*, and from any point *G* of the figure draw the ordinate GD parallel to OA. In AO take any point O, and join OD meeting BL in d. Through d draw dg making any angle with BL, and take

$$dg : dO = DG : DO.$$

Then g will be the point in the new figure corresponding to the point G. Complete the parallelogram $OABa$.

Then $$ad : OA = Od : OD = dg : DG = AB : AD,$$

$$AD = \frac{OA \, . \, AB}{ad}, \quad DG = \frac{OA \, . \, dg}{ad},$$

or writing X for AD, Y for DG, x for ad, y for dg, and m, n for the constants AO, AB, the above relations become

$$X = \frac{m \, . \, n}{x}, \quad Y = \frac{m \, . \, y}{x}.$$

Then if G describes a straight line, the equation connecting X and Y is of the first degree, and so also is the equation between x and y, and therefore the locus of g is a straight line. Similarly if the relation between X and Y is of the second degree, so also is that between x and y, and therefore the lines in which the points G, g are placed are of the same analytical order.

Further, if any straight line touches the curve line in the first figure, the same straight line transferred the same way with the curve will touch the curve in the new figure, and *vice versa.*

By means of this Lemma any straight lines converging to a point are transformed into parallels by taking for the line OA any straight line passing through the point of intersection of the converging lines, which then go off to infinity. So also a straight line and a conic may be transformed into a straight line and a circle, and after the problem is solved in the new figure, if by the inverse operation we transform the new into the first figure, we shall have the solution required.

In Prop. xxv., *to describe a conic given two points and three tangents,* if two of the tangents intersect in A, and the third tangent meets the line joining the two given points in O, then taking the line AO for the first ordinate, and transforming the figure by the Lemma, the two tangents will be parallel to each other, and the third tangent will be parallel to the line joining the two given points.

In Prop. xxvi., *to describe a conic given four tangents and one point,* the line joining the intersection of two tangents to the intersection of the other two is taken as the first ordinate, and the four tangents are transformed into the sides of a parallelogram.

There still remained the case where five tangents are given, and for this Newton required three more Lemmas, xxiii., xxiv., xxv., the second of which had been already given by Apollonius in Bk. III. Prop. 42.

Lemma xxiii. If two straight lines AC, BD given in position, and terminated at the points A, B, are in a given ratio and the straight line CD joining the undetermined points C, D is cut in K in a given ratio, the locus of K is a straight line.

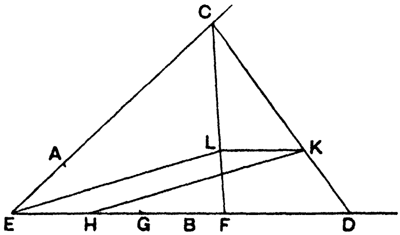


FIG. 21.

Let AC, BD meet in E, and in BE take $BG : AE = BD : AC$, and take $DF = EG$. Then $EC : GD$ (or EF) $= AC : BD$, *i.e.* in a given ratio. Therefore the triangle EFC is of given species. In CF take the point L so that

$$CL : CF = CK : CD.$$

Since this is a given ratio, the triangle EFL is of given species, and therefore the point L will be in the line EL given in position. Join LK. Then the triangles CLK, CFD are similar. And since FD is given, and $LK : FD$ in a given ratio, LK is also given. Taking $EH = LK$, $ELKH$ is a parallelogram. Therefore the point K is always placed in the line HK (given in position) of that parallelogram.

Cor. Because $EFLC$ is given in kind, the three straight lines EF, EL, EC, *i.e.* GD, HK, EC, will have given ratios to each other.

Lemma xxiv. AF, BG, two parallel tangents to a conic, are cut by a third tangent at F and G, and CD is the semi-diameter parallel to AF and BG. Then

$$AF \,.\, BG = CD^2. \quad \text{(Ap. III. 42.)}$$

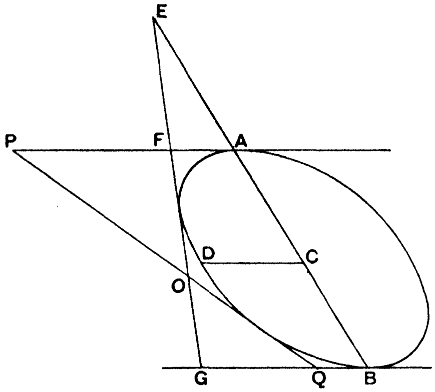


FIG. 22.

Cor. 1. If two tangents FOG, POQ meet the parallel tangents in F, G, P, Q,

$$\begin{aligned} AF : BQ &= AP : BG, \\ &= FP : GQ, \\ &= FO : OG. \end{aligned}$$

Cor. 2. Whence also the two straight lines PG, FQ will meet in the diameter ACB.

Lemma xxv. $IKLM$ is a parallelogram circumscribing a conic, and a fifth tangent cuts the sides of the parallelogram in F, Q, H, E. Then

$$ME : MI = BK : KQ$$

and

$$KH : KL = AM : MF.$$

For by Lemma xxiv. Cor. 1,

$$ME : EI = (AM \text{ or}) \; BK : BQ;$$

$$\therefore \; ME : MI = BK : KQ;$$

also

$$KH : HL = (BK \text{ or}) \; AM : AF;$$

$$\therefore \; KH : KL = AM : MF.$$

Cor. 1. Since the triangles KQH, MFE are similar,

$$\therefore\ KH : KQ = EM : MF;$$

$$\therefore\ KH \cdot MF = KQ \cdot EM = MI \cdot BK = \text{a given quantity.}$$

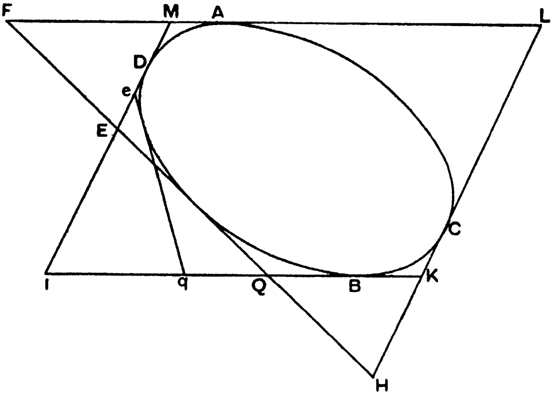


FIG. 23.

Cor. 2. If a sixth tangent eq is drawn meeting the tangents KI, MI in q and e, the rect. $KQ \cdot ME = Kq \cdot Me$, and

$$KQ : Me = Kq : ME, \quad = Qq : Ee.$$

Cor. 3. Hence also, if Eq, eQ are joined and bisected, and a straight line drawn through the points of bisection, it will pass through the centre of the conic. For since $Qq : Ee = KQ : Me$, the same straight line will pass through the middle points of all the lines Eq, eQ, MK by Lemma xxiii., and the middle point of MK is the centre of the conic.

It is worthy of notice that Cor. 2, which proves that

$$KQ \cdot ME = Kq \cdot Me = \text{const.},$$

contains in effect the anharmonic property of the tangents of a conic, viz. *Given two homographic ranges not in perspective on two given straight lines, the lines joining pairs of corresponding points envelop a conic which touches the two given lines.*

Prop. xxvii. Problem xix. To describe a conic given five tangents.

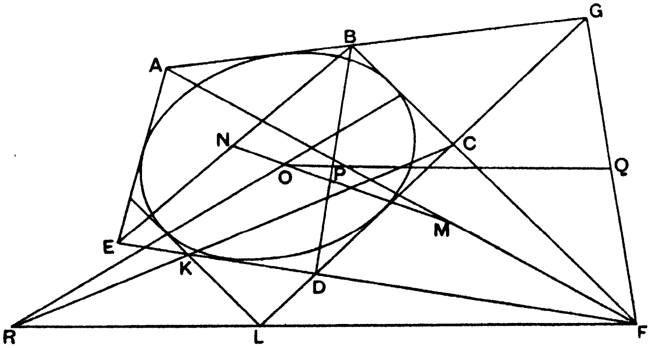


FIG. 24.

Let ABG, BCF, GCD, FDE, EA be the five tangents.

Consider the quadrilateral $ABFE$ contained by four of them.

Bisect the diagonals AF, BE at M and N. Then by Lemma xxv. Cor. 3, MN will pass through the centre O of the conic. Similarly if BD, GF, the diagonals of the quadrilateral $BGDF$, be bisected at P, Q, then PQ will pass through the centre O of the conic. $\therefore$ MN and PQ intersect at the centre O. Parallel to any tangent BC draw KL at such a distance that the centre O may be placed in the middle between the parallels. Then KL will be a tangent. Let KL cut any other two tangents GCD, FDE in L and K. Through the points C and K, F and L, where the non-parallel tangents CL, FK meet the parallel tangents CF, KL, draw CK and FL meeting in R. Then OR will cut the parallel tangents CF, KL in the points of contact by Lemma xxiv. Cor. 2.

In this way the points of contact of the five tangents may be found, and the conic described through five points.

In the above method, which is Newton's, the figure is rather difficult to draw. That given in the following solution is much easier.

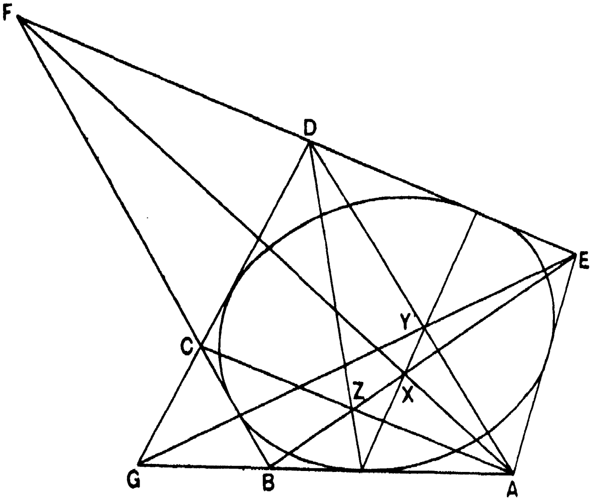


FIG. 25.

Let AB, BC, CD, DE, EA be the five tangents. Join EB and AF meeting in X, and join AD and EG meeting in Y. Then XY produced passes through the points of contact of AB and DE. In the same way may be found the points of contact of the other tangents.

Another method, more simple still, is to join D to Z, the intersection of AC and BE. Then DZ passes through the point of contact of AB.

NEWTON ON PLANE CUBIC CURVES.

By Prof. H. Hilton, D.Sc.

The first edition of Newton's *Optics* was published in 1704. To this work were added two appendices, *Tractatus de Quadratura Curvarum* and *Enumeratio linearum tertii Ordinis*, which were omitted from subsequent editions. Newton explains that the *Tractatus de Quadratura* was published because he had lent out a manuscript containing such theorems and had since met with things copied out of it. He goes on : " I have joined with it another small tract concerning the curvilinear coordinates of the second kind,* which was also written many years ago, and made known to some friends, who have solicited the making it public."

Newton refers to cubics in a letter to Leibniz dated 1676, so this tract on cubic curves may have been written before that time. It contains a classification of cubics into seventy-two species. These are enumerated without proof, and in some cases the argument is obscure. Hence the eighteenth and nineteenth centuries produced many commentators. The first was James Stirling in his *Lineae tertii ordinis Newtonianae*, which was printed in the Sheldonian theatre at Oxford in 1717, during Newton's lifetime. He inserted proofs of Newton's statements and added four species which Newton had overlooked, while Cramer and Murdoch added one species apiece about the year 1746. In 1731 Clairaut and Nicole independently supplied proofs of Newton's statement that every cubic is the projection of some curve whose equation is of the form

$$y^2 = ax^3 + bx^2 + cx + d.$$

In 1861 Talbot translated the tract into English, adding detailed proofs and criticisms.

De Gua was the first to point out that Newton's definition of " species " was not free from ambiguity. In any case his classification was quite arbitrary. He starts from four fundamental types of equation, namely

$$xy^2 + ey = ax^3 + bx^2 + cx + d, \qquad xy = ax^3 + bx^2 + cx + d,$$
$$y^2 = ax^3 + bx^2 + cx + d, \qquad y = ax^3 + bx^2 + cx + d.$$

The cubics represented by the first of these equations have $x=0$ as asymptote and

$$ax^4 + bx^3 + cx^2 + dx + \tfrac{1}{4}e^2 = 0$$

as tangents parallel to this asymptote. They are classified by the reality and position of these tangents, while other species are obtained by giving special values to the coefficients. For instance, if $e=0$, the curve has a " diameter," *i.e.* is symmetrical. Similar methods are applied to the other three cases. That Newton's four equations do really include all possible cubics was first shown by Nicole in 1729.

Since Newton's time other mathematicians have enumerated the various types of cubic using other methods of classification. Thus Cramer gives fourteen species, Euler sixteen, and Plücker two hundred and nineteen. In

---

[*] Newton calls curves of the rth degree " curves of the $(r-1)$th kind."

1762 Waring investigated quartics by the Newtonian method and obtained 84,551 species.

If Newton's work were undertaken for the first time to-day, probably a classification on a projective basis would be adopted, which was hardly natural in 1704. Most mathematicians would be content to distinguish five species: two-circuited, one-circuited, crunodal, acnodal, and cuspidal cubics. It might, however, be thought desirable to differentiate curves according to their relationship to the line at infinity, which is practically what Newton did. The acnodal cubics would then be classified according as the line at infinity met the curve once (at a real point), met it thrice, met the curve only at an inflexion, met it at an inflexion and two other points, was the line of inflexions, was a tangent, was the tangent from an inflexion, passed through the acnode, joined the acnode and an inflexion, or was an inflexional tangent. Similarly we should have nine cuspidal, ten crunodal, eight one-circuited, and eleven two-circuited cubics, giving forty-eight species in all. Two such curves as

$$y^2x+(x+1)(x+2)(x\pm 3)=0$$

would belong to the same species, whereas Newton counted them as belonging to different species. Division into sub-species would be possible. For instance, consideration of the relative positions of branches and inflexional tangents might split up into three sub-species one-circuited cubics whose three real inflexions are at infinity.

To the list of cubic curves Newton adds a note, which is interesting as containing an early example of the use of homographic pencils. Suppose A and B are fixed points, α and β are fixed angles, while $PAD=\alpha$, $PBD=\beta$. Then, if P describes a line, D traces out a conic through A and B. Given five points A, B, H, I, J on a conic, we can construct any number of points on the curve by taking $HAB=\alpha$, $HBA=\beta$ and using I and J to get the linear locus of P, when the locus of D is the conic $ABHIJ$. Again, if P describes a conic through A, D traces out a cubic through B with a node at A; and we can construct any number of points on a nodal cubic, given the node and six other points. In general, if AP and AD trace out homographic pencils, and so do BP and BD, while P describes a curve of degree n, then D traces out a curve of degree $2n$ with three multiple points of order n.

Newton's tract is the first serious contribution to the Theory of Higher Plane Curves, which received its greatest development in the nineteenth century. It is a noteworthy sign of the versatility of his mathematical genius.

NEWTON AND THE ART OF DISCOVERY.

By J. M. Child, B.A.

> *" It is an extremely useful thing to have knowledge of the true origins of memorable discoveries, especially those that have been found not by accident but by dint of meditation. It is not so much that thereby history may attribute to each man his own discoveries and others should be encouraged to earn like commendation, as that the art of making discoveries should be extended by considering noteworthy examples of it."* *

To nobody's work, perhaps, do the above remarks apply with greater force and truth than to the work of Newton ; and this is so not only on account of the interest and importance of the discoveries he made, but also because the sources from which he drew his ideas and the progress of their development can be traced ; moreover, no subject, even those already considered by his brilliant forerunners and contemporaries, when taken up by Newton, failed on completion (whether the method used was partially borrowed or wholly original) to bear the stamp of his genius.

I. Some of the sources that gave occasion for the discoveries of Newton, and the development of the ideas that sprang from these sources, are beyond doubt ; at others we can only guess, supporting conjectures with arguments that are feasible, or merely plausible, according to the bias of the reader. Thus, there is no doubt as to the source from which Newton obtained the ideas which ultimately led to the Method of Infinite Series, the Quadrature of Curves, and the generalization of the Binomial Theorem. For Newton, in a letter sent to Oldenburg for transmission to Leibniz, confesses in precise terms his indebtedness to the *Arithmetica Infinitorum* of John Wallis.

It is commonly stated that the first (or one of the first) infinite series obtained by Newton was that for the inverse sine ; and that he obtained this by expanding $(1-x^2)^{-\frac{1}{2}}$ by means of the Binomial Theorem, and integrating the resulting infinite series term by term. This is hopelessly inaccurate ; and more, it gives quite a wrong idea of Newton's intellectual standard even as an undergraduate. The truth of the matter is that the *Arithmetica Infinitorum* was a landmark in the history of mathematics ; not because of the results that Wallis obtained, but because it raised the principle of Induction to the level of a science. Hitherto, Induction may be looked upon as having had as its motto : " Nature never intended so many results to follow a particular law, unless it was a general law." Wallis, however, proceeded on exactly the same lines as those by which at the present time the laws of algebra are laid down as a generalization of the more or less empirical laws of counting with natural numbers, *i.e.* positive integers. Only two men seem to have had the genius to recognize its true inwardness : one was Euler, who from a consideration of one of Wallis's tables evolved the idea of generalizing the factorial and invented the Beta and Gamma functions, a perfectly simple matter once the generalization had been made ; the other was Newton, who from a consideration of the self-same table, with his eyes turned to the way in which the table had been constructed, was inspired to consider Wallis' finite series as capable of bearing an intelligible meaning if they were indefinitely continued, and the rest was perfectly simple and a natural consequence of what Wallis had proved. Further, all Wallis's work is on the ratios of two infinite series; thus, the source of the idea of the possibility of work with such series is beyond question.

---

* The opening paragraph of Leibniz' *Historia et Origo Calculi Differentialis*.

The letter to Oldenburg referred to above was dated October 24, 1676. After referring in eulogistic terms to Leibniz and certain methods of obtaining convergent series, he wrote:

"One of mine I have already described:* I now add another, that, indeed, by means of which I first obtained these series. For I came upon them *before I knew divisions and extractions of roots*,† which I now use. To explain this I must state the origin of the theorem placed at the beginning of my former letter, as Leibniz desires me to do.

"At the commencement of my mathematical studies, when I came across the work of our excellent Wallis, I considered the series by the intercalation of which he gives the area of the circle and the hyperbola. That is, I observed that in a series of curves, of which the common base or axis is x, and the ordinates applied to it are $\overline{1-xx}|^{\frac{0}{2}}$, $\overline{1-xx}|^{\frac{1}{2}}$, $\overline{1-xx}|^{\frac{2}{2}}$, $\overline{1-xx}|^{\frac{3}{2}}$, $\overline{1-xx}|^{\frac{4}{2}}$, $\overline{1-xx}|^{\frac{5}{2}}$, $\overline{1-xx}|^{\frac{6}{2}}$, &c., if the areas of the alternates, which are x; $x-\frac{1}{3}x^3$; $x-\frac{2}{3}x^3+\frac{1}{5}x^5$; $x-\frac{3}{3}x^3+\frac{3}{5}x^5-\frac{1}{7}x^7$, &c., could be interpolated, we should have the areas of the intermediates, of which the first, $\overline{1-xx}|^{\frac{1}{2}}$, is the circle: for the interpolation of these, I noted that in all of them the first term was x, and that the second terms, $\frac{0}{3}x^3$, $\frac{1}{3}x^3$, $\frac{2}{3}x^3$, $\frac{3}{3}x^3$, &c., were in Arithmetical Progression; and thus, that the first two terms of the series to be intercalated ought to be $x-\frac{\frac{1}{2}x^3}{3}$, $x-\frac{\frac{3}{2}x^3}{3}$, $x-\frac{\frac{5}{2}x^3}{3}$, &c.

"To intercalate the remaining terms, I considered that the Denominators 1; 3; 5; 7; &c., were in Arithmetical Progression: and thus only the numerical coefficients for the numerators had to be found. Moreover, these in the given alternate areas were the figures of the powers of the undenary number; namely, 11^0; 11^1; 11^2; 11^3; 11^4; *i.e.* first, 1; then 1, 1; thirdly, 1, 2, 1; fourthly, 1, 3, 3, 1; fifthly, 1, 4, 6, 4, 1; &c. So I sought the manner in which, for these series, the remaining figures could be derived from the first two; and I found that, taking the second figure to be m, the remaining figures were produced by continued multiplication by the terms of the series,

$$\frac{m-0}{1}\times\frac{m-1}{2}\times\frac{m-2}{3}\times\frac{m-3}{4}\times\frac{m-4}{5}\text{; \&c.}$$

(Here follow examples of the use of this continued product.)

Hence I saw that the required Area of a circular segment was

$$x-\frac{\frac{1}{2}x^3}{3}-\frac{\frac{3}{8}x^5}{5}-\frac{\frac{1}{16}x^7}{7}-\frac{\frac{5}{128}x^9}{9}\text{; \&c.}$$

"By similar reasoning there also came forth the intercalated areas of the remaining curves: as also the area of the hyperbola and the other alternates in the series $\overline{1+xx}|^{\frac{0}{2}}$; $\overline{1+xx}|^{\frac{1}{2}}$; $\overline{1+xx}|^{\frac{2}{2}}$; $\overline{1+xx}|^{\frac{3}{2}}$; &c. The method is the same for intercalating other series; and that too, whether through an interval having two or more terms missing at the same time."

Let us pause here for a moment to make a few observations. According to Newton's own statement, his first infinite series was not the equivalent of

---

* This was the enunciation of the Binomial Theorem in a letter to Oldenburg, dated June 13, 1676, to be communicated to Leibniz (*eo mediante*). This was sent on to Leibniz, dated July 26, and drew from Leibniz a reply, dated August 27, requesting Oldenburg to ask Newton to send further particulars and explanations.

† The italics are mine.—J. M. C.

$\int_0^x dx/\sqrt{1-x^2}$, but that of $\int_0^x \sqrt{1-x^2}\,dx$; we see that the Binomial Theorem was not used (we shall see later that it was not known to Newton at the time), and that there is no trace of integration term by term. It is a direct outcome of a consideration of the work of Wallis ; and Newton's statement of indebtedness, frank as it is, hardly conveys the true measure of what he owed to his study of Wallis.

In Prop. CLXIX, Wallis gave a table of figurate numbers ; in Prop. CLXXVIII, he showed how these may be interpolated by finding formulae for triangular, pyramidal, triangulo-pyramidal, &c., numbers, *defining indices in such a way that these formulae may be taken to be general* ; in Prop. CLXXXIV, he showed how the table could be filled up more easily by Pascal's rule of addition, the rule of "the one above and the one before" ; and he remarks in a note : "Moreover, all that has been said in several of the preceding propositions with regard to the interpolation of one number in each interval can be adapted easily *to the interpolation of two, three, or more numbers in each interval,* with suitable alterations." In Prop. CLXXXVIII, he showed how the numbers in the table can be formed from one another by the use of continued products, one for the even members and one for the odd members of each row, and that these can be generalized and hold also for the columns.

Thus all that was left for Newton to do was to give each row a shift of two spaces to the right (whether he did this or not there is no indication, but it makes things very much easier), to alter the formulae to correspond, and to make out a general formula for all the columns. He would have had the table below :

	INDEX.	0/2	1/2	2/2	3/2	4/2	5/2	6/2	7/2	8/2
Coefficients of	x	1	1	1	1	1	1	1	1	1
	$-x^3/3$	0	1/2	1	3/2	2	5/2	3	7/2	4
	$-x^5/5$		A	0	3/8	1	15/8	3	35/8	6
	$-x^7/7$		B		C	0	15/48	1	105/48	4
	$-x^9/9$							0	105/348	1
	$-x^{11}/11$									0

and so on.

The rules given by Wallis, altered to correspond, are :

(*a*) the first row is a series of units, and the second row the Arithmetical Progression, 0, ½, 1, &c. ;

(*b*) insert the zeros above the zig-zag line drawn across the diagram ;

(*c*) each number in the table is the sum of the number two spaces to the left of it and the number above the latter ;

(*d*) in the $(m+1)$th line, the number next to the right of the zero is the generalized figurate number, $\frac{n}{1}\cdot\frac{n+1}{2}\cdots\frac{n+m-1}{m}$, where (for this table) $n=\frac{1}{2}$;

(*e*) the law of the columns, where m is the number in the second line, is

$$1\times\frac{m}{1}\times\frac{m-1}{2}\times\frac{m-2}{3}\times\frac{m-3}{4}\times\text{\&c.}$$

It is obvious that this table can be extended indefinitely to the right, downwards, and to the left; thus, $\frac{3}{8}$ is the sum of A and the figure above it, $\frac{1}{2}$, and hence $A=\frac{3}{8}-\frac{1}{2}=-\frac{1}{8}$; $C=\frac{15}{48}-\frac{3}{8}=-\frac{1}{16}$; and $B=-\frac{1}{16}-(-\frac{1}{8})=+\frac{1}{16}$; and so on.

Similarly for two interpolates, the rules are the same except that in (*c*), *three* has to be substituted for *two*; and in (*d*), for the number to the right of the zero $n=\frac{1}{3}$ instead of $\frac{1}{2}$, and for the number to the right of this $n=\frac{2}{3}$; and for the second row the Arithmetical series 0, $\frac{1}{3}$, $\frac{2}{3}$, 1, &c., must be used. Similarly, a slight change in the rules is required for four, or more, interpolates.

As all these early mathematicians are known to have been very conscientious, and to have done a great deal more verification than appeared in their published work, I suggest that Newton actually carried out the extensions of the tables before he had satisfied himself of the reliability of the continued product formula. This is the more probable because Newton, like Wallis, in his early days, seems to have been very fond of elaborate calculations; for, speaking of his calculation of logarithms by means of the infinite series for the area under the hyperbola, Newton says: "I am ashamed to say to how many places of figures I carried these computations, having leisure at that time."

It is clear that it could not have escaped the notice of anyone who had studied Wallis to such good purpose, that the latter, in Prop. LXIV, had given the general rule for the area under a curve whose ordinate is x^n in the form $x^{n+1}/(n+1)$, or, as Newton puts it:

$$\text{“ Si } ax^{\frac{m}{n}}=y\text{; Erit } \frac{an}{m+n}x^{\frac{m+n}{n}}=\text{Area } ABD.\text{” *}$$

Hence it follows that the whole of the early work of Newton was founded on a perfectly sound basis, an induction carried out *scientifically* by Wallis, by analogy and generalization of definitions; and this is a totally different matter from the unscientific method of "integration term by term of an infinite series" without modern safeguards, which is usually stated to have been the way in which Newton obtained his first results.

There is even internal evidence that Newton concerned himself with at least the elementary principles of convergence as affecting the infinite series he used. For, in the *De Analysi*, which was sent to Barrow in July, 1669, Newton gave as an example the determination of the area under the curve whose ordinate is the reciprocal of $(1+xx)$.

"In the same way, if $\frac{1}{1+xx}=y$, by division there will come forth

$$y=1-x^2+x^4-x^6+x^8, \text{ \&c.}$$

Whence (by the second rule) we shall have $ABDC=x-\frac{1}{3}x^3+\frac{1}{5}x^5-\frac{1}{7}x^7+\frac{1}{9}x^9$, &c. Or, if the term xx be placed first in the divisor, viz. thus $(xx+1)$, there will come forth $x^{-2}-x^{-4}+x^{-6}-x^{-8}$, &c., for the value of y. Hence (by the second rule) we shall have $BDa=-x^{-1}+\frac{1}{3}x^{-3}-\frac{1}{5}x^{-5}+\frac{1}{7}x^{-7}$, &c. Proceed by the first method when x is sufficiently small, and by the second when x is sufficiently large."

Lastly, let us consider the words I have italicized above in the extract from the letter to Oldenburg: "For I came upon them before I knew divisions and subtractions of roots." Compare these words with the closing sentence of the extract: "The method is the same for intercalating other series; etc." Was there any other table given by Wallis, from which Newton by interpolation could have obtained any of the series that he was known to have used in his early work, without using divisions or extractions of roots? For instance, could he have obtained the logarithmic expansion, as the area under the equilateral hyperbola, which he is known to have used for calculation in the years 1665-1666? In Prop. CLIX *et seq.*, Wallis gave the areas under curves whose ordinates come under the general formula, $(\sqrt[n]{R} + \sqrt[n]{ar})^m$; the arithmetical work showed that the general formula is

---

* The opening paragraph of *De Analysi per æquationes numero terminorum infinitas.*

$$n\left(\frac{A}{n} + \frac{B}{n+1} + \frac{C}{n+2} + \ldots + \frac{K}{n+m}\right),$$

where $A, B, \ldots, K$ were clearly to be obtained by the use of the continued product $1 \times \frac{m-0}{1} \times \frac{m-1}{2} \times \frac{m-2}{3} \times$ &c., *a continued product that had been found and used by Newton for his first series.* The area of the hyperbola is obtained, by putting $n=1$ and $m=-1$, in the form $x - \frac{1}{2}x^2 + \frac{1}{3}x^3 - \frac{1}{4}x^4$, &c.; for Wallis's R and ar are, as usual, Newton's 1 and x.

Thus, it is quite probable that Newton did obtain the logarithmic expansion in this way before "he knew divisions and extractions of roots." I prefer to think that he did; for it has an important bearing. It is to be noticed that it is a case of *exterpolation*, which Wallis had used in several tables, and this may provide the clue to the reasoning which, in the formulation of the Binomial Theorem in its integrated form, led Newton to extend the table given above *to the left*; for, clearly this was not necessary to obtain the series mentioned above as being derived from it, but it is necessary for the case of the negative index for the Binomial Theorem.

II. We will now consider the formulation of the Binomial Theorem itself, how it was discovered, and the probable date of its discovery. First of all, it is to be noted that, in the *De Analysi* sent to Barrow in 1669, use is made of Division and Extraction of Roots, together with the Method by Solution of Adfected Equations; but there is no mention of the Binomial Theorem. Newton's "RULE THREE" for the "Quadrature of all other Curves" is thus stated:

"*If the value of y, or of any term of it, is more composite than those given above, it must be reduced to simpler terms: by operating with letters in the same manner as that by which arithmeticians divide in decimals, extract roots, or solve adfected equations: and the required area of the curve you will then obtain by the preceding rules.*"

It is hard to believe that this tract of 1669 did not contain all that Newton knew at the time; especially as, on the receipt of a copy of Mercator's *Logarithmotechnia*, in which the area under the hyperbola was found by terms of the logarithmic series, it was written to acquaint Barrow and others that Newton had a general method for all curves, of which the series used by Mercator was only a special case.

It would appear that the first authentic statement of the Binomial Theorem itself, generalized to include the case of the fractional index, but apparently not that of the negative index (although, as stated above, that would not be long in forcing itself on Newton's attention, once the first generalization had been made), was made in the "first" letter to Oldenburg, dated June 13, 1676, in the following form:

$$(P+PQ)^{\frac{m}{n}} = P^{\frac{m}{n}} + \frac{m}{n} AQ + \frac{m-n}{2n} BQ + \frac{m-2n}{3n} CQ + \frac{m-3n}{4n} DQ + \&c.,$$

where in the second term A stands for the first term, in the third term B stands for the second term, and so on. Further, in the "second" letter to Oldenburg, dated October 24, 1676, immediately following the extract from it given above, there occurs the following passage: "This was my first entry into these meditations; and truly I should have forgotten it, but that, *some weeks ago,* I once more referred to certain notes (*adversaria*).* But when I had considered these, I *immediately* perceived that the terms $\overline{1-xx}|^{\frac{0}{2}}$, $\overline{1-xx}|^{\frac{2}{2}}$, $\overline{1-xx}|^{\frac{4}{2}}$, $\overline{1-xx}|^{\frac{6}{2}}$, &c. (that is, 1, $1-xx$, $1-2xx+x^2$, $1-3xx+3x^2-x^3$, &c.) could be interpolated by the same method as the areas generated by them; and that nothing further was required beyond the omission of the denominators 1, 3, 5, 7, &c., in the terms expressing the areas. That is, the coefficients of the quantity to be intercalated, $\overline{1-xx}|^{\frac{1}{2}}$, or $\overline{1-xx}|^{\frac{3}{2}}$, or generally $\overline{1-xx}|^{m}$, were produced by the continual multiplication of the terms of the series

---

* Very probably while preparing the first letter.

$$m \times \frac{m-1}{2} \times \frac{m-2}{3} \times \frac{m-3}{4} \times \text{\&c.}$$

And so (for example), $\overline{1-xx}|^{\frac{1}{2}}$ would come to $1-\frac{1}{2}xx-\frac{1}{8}x^4-\frac{1}{16}x^6$, &c.; and $\overline{1-xx}|^{\frac{3}{2}}$ would come to $1-\frac{3}{2}xx+\frac{3}{8}x^4+\frac{1}{16}x^6$, &c.; and $\overline{1-xx}|^{\frac{1}{3}}$ would come to $1-\frac{1}{3}xx-\frac{1}{9}x^4-\frac{5}{81}x^6$, &c. And thus became known to me a general reduction of radicals to infinite series, by the rule which I placed at the beginning of the former letter, *before that I had found the extractions of roots.*"

I consider that the words which I have italicized above, *some weeks ago*, *immediately*, *before . . . roots*, demonstrate clearly that the date of the discovery of the Binomial Theorem itself was approximately the date of the first letter to Oldenburg, *i.e.* the year 1676. The fact that the square root of $aa+xx$ is found in the *De Analysi* of 1669, *more arithmetico* as described in "Rule Three," in my opinion precludes the possibility of the Binomial Theorem having been discovered in 1665, as stated by Rouse Ball.

Of course, it is of no real importance whether it was discovered in 1676 or 1665; for Newton was using it in its integrated form from the beginning of his studies, and so probably before 1665. And, of course, my remarks apply only to the generalized form, and not to the form for positive integral indices, which in the form of the Arithmetical Triangle and the rule of "one above and the one before," was known to Pascal. But it is more in accord with my conception of the genius of Newton to think that the theorem lay latent, until he had occasion to consider some problem which demanded more than the square-root process *more arithmetico*, and had referred to his notes to see how he had dealt with such things previously; and then, with the fuller insight which he then had, he *immediately* perceived not only the theorem itself for fractional indices, and its practical importance, but was later led to develop the theorem for negative indices to avoid division by an infinite series which had been used previously. We see how slight an impetus was needed to set the intellect of Newton working towards wonderful developments.

III. We have next to consider the source from which Newton developed the Calculus; and by this term I mean the calculus of his later work, and not the early fluxions or the Wallisian method of integration as embodied in the rule: "If $ax^{\frac{m}{n}}=y$, then $\frac{an}{m+n}x^{\frac{m+n}{n}}=\text{Area}$."

I have no doubt that the source was the intercourse between Barrow and Newton at the time when Barrow was preparing the *Lectiones Geometricae* for the press.

The years between 1661, when Newton went to Cambridge as an undergraduate, and 1669, when he became Lucasian Professor in succession to Barrow, may be divided into periods: (1) 1661 to 1665, his undergraduate days; (2) 1665 to 1667, when he returned to Cambridge after the enforced absence due to the plague, and became a Fellow of his college; and (3) 1667 to 1669, when he again came into intimate contact with Barrow. As an undergraduate he attended the lectures of Barrow; these, however, were those published in 1683 under the title of *Lectiones Mathematicae*, though delivered in 1664-5-6;

and were philosophical lectures on the fundamentals of mathematics, having little to do with infinitesimal Calculus. The *Lectiones Geometricae,* published in 1670 as an appendix to the *Lectiones Opticae,* do not seem to have been publicly delivered as university lectures ; but may have been given as college lectures, or the principles may have been explained to competent pupils. I have shown that these *Lectiones Geometricae* * are an *organized* and *complete* treatise on the Calculus, though cast in a *practically useless* geometrical form : that they contain theorems equivalent to the differentiation and integration of all the ordinary functions to be met with in the elementary text-books of to-day, together with the rules for the differentiation of a sum, a product, a quotient, or a function of a function, the proof of the inverse nature of the operations of differentiation and integration, a practical method for the integration of a function not coming under one of the standard forms, and the fundamental theorem for rectification of an arc. In this work occurs the method of the differential triangle ; its explanation is introduced by the words : " a method for finding tangents by calculation frequently used by me " (*a nobis usitatum*) . . . " I do so on the advice of *a friend* " (Newton, as we learn from the preface) ; " and all the more willingly, because it seems to be more profitable and general than those which I have discussed."

Now Barrow's Rules are :

1. " In the calculation, I omit all terms containing a power of a or e or products of these (for these terms have no value)."

Barrow's a and e are the increments of y and x respectively, and equivalent to Newton's $\dot{y}o$ and $\dot{x}o$; the whole rule is equivalent to Newton's Rule II, second part, and Rule III.

2. " After the equation is formed I reject all terms consisting of letters denoting known or determined quantities, or terms which do not contain a or e (for these terms if brought over to one side of the equation will always be equal to zero)."

This is Newton's Rule II, Part I, which requires the original equation to be subtracted.

3. " I substitute m, the ordinate, for a, and t, the subtangent, for e. Hence the length of the quantity of the subtangent is found."

This is equivalent to Newton's last step, the obtaining of the ratio of $\dot{x}o$ to $\dot{y}o$, which is exactly the same as Barrow's ratio of e to a.

The similarity is too close for the two methods to have been independently discovered ; and we have to face the possibility that Newton was the original discoverer, and that he communicated it to Barrow, whereupon Barrow inserted it in the *Lectiones* in his own fashion. This hardly seems probable ; for Barrow was very conscientious in ascribing credit wherever credit was due to previous workers, and yet the method is described in the first person singular. Further, I think that the words *a nobis usitatum* constitute a confession on the part of Barrow as to the means whereby he obtained most of his geometrical theorems ; namely, by first working the results out analytically, *i.e.* by calculation with the a and e method, and then translating them into geometry.† Barrow could not help being struck by the fact that a man of Newton's calibre, one who could master Descartes' *Géométrie,* should be so poor at pure geometry ; and I suggest that, even if they were not the subject of college lectures, he would have shown him some of his geometrical theorems, and have explained how he had been led to them. The analytical character of the method would have appealed to Newton very forcibly, if this were the case.

---

* Open Court Co., 1916.

† In his work on Apollonius, Barrow states that, in his opinion, Apollonius and Archimedes must have used a preliminary algebraical analysis ; and I think that this is really a statement of what Barrow himself had done.

Then he had to leave the university, and there followed that second period of two years, which were so fruitful—*largely, I have no doubt, because he was away from the geometrical influence of Barrow.* Within a few months, by November 13, 1665, he had so far perfected the method of fluxions that he was able to find the radius of curvature of any curve at any point, and within another year he was applying the method to problems on the theory of equations. These labours formed the groundwork of the *De Analysi*, which was communicated to Barrow in July, 1669. Previous to this communication, however, he must have acquainted Barrow with his application of the method of fluxions to infinite series; for there is the following passage in a letter to Collins, dated December 10, 1672: "I remember on one occasion that I told Barrow, when he was engaged in preparing his lectures" (query ?, for press—or maybe, as I have suggested, getting them together with a view to delivering them), "that I was in possession of such a method for drawing tangents; but I do not know by what digression *I was prevented from describing the method to him.*"

However, at the time of the *De Analysi*, he could only differentiate by means of the o-method, and his integration consisted merely of the Wallisian rule. There is one note, however, which may be taken to mean a real integration; Newton says: "If $\sqrt{aa+xx}=z$ (the area), we get by calculation $\frac{x}{\sqrt{aa+xx}}=y$." But it is almost certain, by what has immediately preceded the note, that the method of obtaining this result was as follows:

If $\sqrt{aa+xx}=z$, then $aa+xx=zz$;

hence $aa+(x+o)(x+o)=(z+oy)(z+oy)$;

$\therefore\ 2x=2zy=2\sqrt{aa+xx}\,.\,y$; whence the result.

Thus in 1669, wonderful as is the work that has been accomplished, yet the methods employed are merely a correlation of what he has learned from Descartes, Wallis and Barrow, combined with his original methods of infinite series and reversion of series. In the true sense of the word, he could not differentiate or integrate, except, as we say nowadays, "by first principles."

Where then does the marvellous development of later years come from? I suggest as the source, Barrow's *Lectiones Geometricae*; as the opportunity, Newton's help in preparing them for press; as the occasion, *the removal of the hampering geometrical influence of Barrow*, who in 1670 turned to theology instead of mathematics; as the spur, the problems on gravitation and the preparation of the *Principia*. For, as I have shown in my little edition of Barrow's *Lectiones*, it is possible, using Barrow's theorems as they stand, to solve geometrically the following problem set me as a test problem by the late P. E. B. Jourdain:

Given any four functions, represented by curves, and the ordinates and subtangents for any abscissa, it is required to draw the tangent for this abscissa to the curve whose ordinate is the sum (or difference) of the square root of the product of the ordinates of the first two curves and the cube root of the quotient of the ordinates of the other two curves.

For example, in other words, differentiate

$$y=\sqrt{\sin x\,.\,\log_{10}(\cos x)}\pm\tfrac{1}{10}\sqrt[3]{3\tan x\div x^2}$$

in a fashion that, step by step shall follow, and be translatable into, the steps ordinarily adopted nowadays.

I contend that it did not require the genius of a Newton once freed from the incubus of geometrical ideas to form from Barrow's theorems a complete analytical elementary "calculus."

In a letter to Collins, mentioned above, dated December 19, 1672, we see the firstfruits of independent work in the shape of what is practically direct differentiation of the equation

$$x^3-2xxy+bxx-bbx+bby-y^3,$$

by means of "multiplication by an arithmetical progression."

"Multiply the terms of the equation by any arithmetical progression according to the dimensions of y, say

$$\begin{array}{cccccc} x^3 & -2xxy & +bxx & -bbx & +bby & -y^3\,; \\ 0 & 1 & 0 & 0 & 2 & 3 \end{array}$$

and also according to the dimensions of x, say

$$\begin{array}{cccccc} x^3 & -2xxy & +bxx & -bbx & +bby & -y^3. \\ 3 & 2 & 2 & 1 & 0 & 0 \end{array}$$

Then the length of the subtangent is

$$\frac{-2xxy+2byy-3y^3}{3xx-4xy+2bx-bb}.\text{''}$$

This Newton describes as a corollary of the general method.

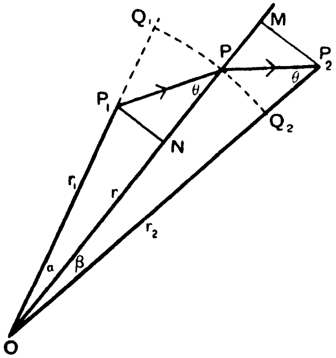
This seems to me the first great step that Newton took, working on his own original lines; what it leads to in the *Principia* is concealed, because Newton, in accordance with the custom of the time, chose to write that work in geometrical form. But in all probability, many of the results were first worked out by advanced differentiation and integration, properly so-called. For instance, in one of Newton's theorems there is a piece of necessary "proof-construction," which is anything but obvious; in the edition of the *Principia* by Le Sueur and Jacquier, the authors give a note in which they endeavour to show how Newton arrives at this proof-construction. Their explanation is exceedingly cumbrous, is not altogether sound, and is geometrical; whereas it is easily shown to follow in three steps, the first using the o-method, the second a pure differentiation and subsequent integration, and the third using pure geometry, the whole taking about ten lines. This "elegant method," the method of mixed proof, seems to me, if I have correctly reproduced Newton's work, to be such as only Newton, among his contemporaries, could have attained; and it was missed by the authors of this edition of the *Principia*, because they did not fully realize the power of Newton. The same lack of understanding seems to have characterized all Newton's "expounders" in the eighteenth century. They did not realize, for one thing, the meaning of Newton's words: "neque antequam, neque postea, sed tunc"; which show that he had the true idea of the gradient of the tangent as being defined by an upper and lower sequence of secants, to neither of which it belonged. This, too, possibly came from Barrow, whose differential triangle is a *curvilinear* triangle, the "triligne" of Pascal, and whose tangent is a straight line meeting the curve in one point and lying wholly on one side of the curve, and not a line that for an infinitely short portion of it *is a secant.*

IV. The attempt to find the source of Newton's ideas on the principles of Mechanics rests on still less satisfactory grounds than the attempt to place the origin of his ideas of the Calculus; for there are so many possible sources. Doubtless, in his undergraduate days he studied books on applied mathematics as well as books on pure mathematics; but unfortunately we have no record of the former such as we have of the latter. It wanted, however, nothing but a Newton to organize, like a latter-day Euclid, the knowledge of the time. Since the appearance, in 1605, of Simon Stevin's *Wisconstige Gedachtenissen* (2 vols. folio, Leyden), of which a Latin translation, under the title *Hypomnemata Mathematica*, was published by Snell in 1608, most of the eminent mathematicians—Galileo, Torricelli, Pascal, Huygens, Wren, to name only a few—had turned their attention to applied mathematics. The editions, in 1669, 1670, of works by Wallis on Statics and Dynamics, formed a fairly complete summary of the extent of the knowledge of applied mathematics up to the date of their publication; and doubtless, Newton, having found such great inspiration in Wallis' *Arithmetica Infinitorum*, would have turned to these to see what he could find of value. But Wallis's work could not have been the source of Newton's first ideas; for at least five years previous to the publication of these works, Newton is known to have been occupied with the subject of gravitation. The occasion for his work is clearly a study of Kepler's Three Laws; and, provided that Newton accepted the law of equable description of areas, which other mathematical contemporaries seem to have doubted, the proof of central acceleration would have been in Newton's hands after five minutes with his method of fluxions. Leonardo da Vinci is credited with knowing the parallelogram of forces; and it is quite possible, since it comes out quite easily from the work of Archimedes by Leonardo's idea of a "potential lever." Even if this were not the case, Stevin had shown that a force could be, as we say nowadays, resolved in two directions at right angles to each other; he also attained to an insight into the principle of "virtual velocities" or "virtual displacements." Galileo had still further developed the latter idea in the principle that in equilibrium the centre of gravity is as low as possible. Galileo had perceived that the idea of inertia involved the principle that a force produces acceleration. Hence, assuming that Newton had cognizance of these more or less hazy ideas, the following proof of central force would be another of his *immediate* perceptions.

Let P be the position of a planet at any time with respect to the sun at O. Let P_1, P_2 be the positions at a short unit of time, o, before and after its arrival at P. Then the velocities before and after the arrival at P are given by

$$P_1P = v_1o \quad \text{and} \quad PP_2 = v_2o.$$

Hence, if the perpendiculars P_1N, P_2M are drawn, these represent the resolved part of the velocities multiplied by o, just before and just after the arrival at P.



Now, if Kepler's law is true, the triangles OPP_1, OPP_2 are equal in area, and hence $P_1N = P_2M$; *i.e.* there is no *change in the velocity at right angles to OP*; that is, there is no acceleration in this direction, and therefore no force. Hence the force must be central.

Such a proof demands the correct formulation of the parallelogram of forces, the determination of the connection between force and acceleration, the further development of the theory of fluxions—in short, the whole preliminary matter of the *Principia*. Did Newton get any inspiration for this development?

The following is frankly offered as sheer guess-work, but I shall consider that this guess-work is justified if, after a careful search into the works that were in Newton's library, there is found a copy of a certain book, especially if it shows signs of having been much used, or if in any of his manuscripts a certain name occurs; and will gladly acknowledge the error of my ways if such a book is not to be found. The book is *De Motu*, and the author is *Baliani*, a name that none of the historians except Mach seems to know, at any rate as having done anything worth mentioning.

Baliani was a Captain of Archers in Genoa, and only an amateur mathematician; so that one cannot expect his material to be very clearly stated. He published the first book of his *De Motu* in 1639, the same year as Galileo's *Two New Sciences.* He opens brilliantly with the experimental fact that the lengths of pendulums vary as the squares of their times of oscillations; then, either making an erroneous assumption of fact or, as I prefer to think, a correct theoretical assumption of principle, he considers that this holds good no matter what the arc of swing may be. He considers an *infinitely long* pendulum moving from a horizontal position; this, in the *prime* part of its motion (by which he means the first infinitesimally short period), has its bob falling vertically under the sole influence of its weight; and for any other position of release, its prime motion is down an inclined plane under the influence of gravity and the perpendicular reaction of the plane, here replaced by the tension of the supporting string. In these theorems, Baliani uses the principle of "phoronomic similarity," afterwards developed by Newton; and not, as Mach says, first presented by Newton. It follows quite naturally that the spaces described are proportional to the squares of the times. Many of the Galilean propositions then follow. Eight years later he published the other five books, of which two are on solids and three on fluids. The second book earned for him the condemnation of Christian Wolf and later of Montucla, who state that Baliani altered his views, and here proved that the spaces traversed are proportional to the times in which they have been attained. They do not perceive that Baliani is philosophizing and trying to explain why the spaces should be proportional to the squares of the times; and that what Baliani is working with is the spaces due to what he calls "impetus" *irrespective of gravity,* that the times are infinitesimal portions of a short period from rest; in short, that Baliani is giving an exact analogue of the modern proof in elementary school text-books, where the time t is divided up into n equal parts, τ, so short that the velocity can be taken to be constant for the interval. According to the philosophy of his day, the effect of gravity was a succession of short jerks occurring very rapidly; and Baliani shows that on this hypothesis the square law is not exact, but approximation is the more exact the more rapid the succession of the small jerks. He says in definition: "Impetus is the propensity (the correct translation of *vis* at this period, not *force*) by which a movable body is apt to proceed without the action of gravity or any other thing." Here is contained the idea of inertia, and of momentum, and (since Baliani usually considered a body of unit mass) velocity, all inextricably mixed together. Note that the same confusion of ideas is inherent in Newton's word "Motio," which was generally misunderstood by his contemporaries; but which to Newton meant, clearly and definitely, the small space traversed in small unit time after a force had acted on unit mass for unit time; *i.e.* the exact idea conveyed by the modern translation "rate of change of momentum." The use of the word "impetus" afterwards in several propositions shows that Baliani had the idea of "conservation of linear momentum"; and we have here nothing lacking but the clear intellect of Newton to formalize the idea into the *First Law of Motion.*

There is very interesting material in the other books, in which, by considering fluids to be composed of particles moving independently of one another, Baliana obtained the flow of fluids in channels, and finally proved Torricelli's Theorem for the flow of water from an orifice. But it is not to the matter of the books so much as to the philosophical prefaces that we must look for the ideas that may have been sources of inspiration to Newton. Thus, in describing an experiment, in which he had two balls dropped for him, one of iron and one of wax, *of the same size*, from a height of fifty feet, and found that the iron ball reached the ground while the wax ball was still about a foot from it, he explains that, "unless I mistake," the reason is that the air has resisted the one more than the other. The result of this experiment shows accuracy on the part of Baliani, for the wax ball, assuming it to have been beeswax, and taking the law of resistance to vary as the square of the velocity on the average, should have fallen 48·7 feet while the iron ball fell 50 feet. In this passage Baliani distinguishes between *moles* and *pondus*, and at this time the former was losing its meaning of size and acquiring something equivalent to Newton's idea of mass as "amount of matter." In other prefaces Baliani attempts to explain why a moving axe will make a deep cut in a block of wood, whereas a huge weight laid on the axe at rest upright on the block will hardly make any incision at all. He explains why a racket strikes a ball further than a flat piece of wood; and his explanation of the "break" on a ball, when it reaches the ground after being thrown against a vertical wall, contains the principle of the "conservation of angular momentum." He contends that on impact with the wall obliquely, the point of contact is stopped, but the impetus of the centre persists; this causes a spin, and the impetus of this spin persists, and causes, on contact with the ground, a deflection from the vertical plane of motion after leaving the wall. These are only a few of the striking points made by Baliani; and although, naturally enough, they suffered through lack of the professional mathematician's lucidity of statement, they would be clear to a Newton. I have no hesitation in saying that, if Newton ever read Baliani's work, there is no need to look any further for the sources of his inspiration; the whole of his fundamental principles would be another of his *immediate* deductions. Newton merely had to gather in the rich harvest in Baliani's field, and thrash it and grind the corn.

The Laws of motion once formulated, and the notion of acceleration of motion in a curved path firmly established, the way is open to planetary motion. Not, as is usually suggested, by a consideration of approximate motion in a circle; from which we have that the velocity is proportional to radius/time, and hence, on account of Kepler's Third Law, proportional to $(\text{radius})^{-\frac{1}{2}}$; from which it follows that the acceleration, v^2/r, is proportional to the inverse square of the radius. This possibly was the manner in which Halley, Wren and others acquired the idea. I suggest, however, that Newton came to it by a consideration of the figure given above, for he would not wish to assume the orbit as circular; and further, the method has in it the germ of fluxions. Thus, since the triangles P_1NP, PQ_2P_2 are ultimately similar, $PN-PM$, which is proportional to the acceleration, is

$$r\beta\,[\cot\theta-\cot(\theta+\beta)], \quad \text{or} \quad r\beta^2/\sin^2\theta, \quad \text{or} \quad PP_2{}^2/OP.$$

But $PP_1/PP_2=r_2/r=r/r_1=r_2{}^{\frac{1}{2}}/r_1{}^{\frac{1}{2}}$, and therefore $PP_2{}^2$ varies inversely as OP. Hence the acceleration varies inversely as the square of the distance. Thus this rough preliminary has merely to be made sound to provide the proofs given in the *Principia*. From the same figure it can be shown that the curve has some of the properties of the ellipse.

The proofs are simple and more or less direct; they follow naturally from Newton's method of small quantities, and account for his perfect conviction that the orbit was elliptic, when Halley, Wren and others had failed to find it (Newton told Halley that he had proved this to be the case in 1679). Hence I suggest that Newton came to his knowledge of it by means of some such figure as I have given above, assuming only the equable description of areas. Further, this is the natural way of approaching the propositions as they appear in the *Principia*.

That man is well on the way to being a genius who can take a second man's subject, apply to it a third man's method, and get more out of either than the originators. How then shall we estimate the intellect of Newton, who, finding comparatively flimsy foundations, instead of merely strengthening them and erecting on them such a building as they can sustain, digs them out, goes down to bed-rock, lays his foundations securely, and then erects a noble edifice, beautiful within and without—an imperishable proof of unparalleled genius.

NEWTON'S INFLUENCE ON METHOD IN THE PHYSICAL SCIENCES.

BY PROF. A. E. HEATH, Ph.D.

I

Two centuries after Newton, we live in a disillusioned age. This does not necessarily mean that we underrate his great achievement ; it does, however, mean that we are prepared to estimate it coolly. We cannot regain the first fine rapture which could hail him as a demi-god ; * nor ask, with de l'Hôpital, if he ate, drank and slept as other men. But we are yet ready to recognize the giant he was ; and perhaps all the better able to appreciate his greatness for not refusing to see the spots on the sun. This characteristic of our time is reflected in the kind of biography we enjoy. We are not satisfied with anything short of the real man. We want to be told the worst, as well as to know the best. And it is, perhaps, significant that when a Lytton Strachey has done his worst on his subjects we are left with the quite definite feeling that the characteristic defects upon which he insists are the very factors through which we are enabled to understand the elements of greatness in the moving and living human beings portrayed.

Now I believe that it is possible to show that a deeper understanding of some aspects of Newton's influence on method in the physical sciences is brought about if we recognize the presence of particular weaknesses in both the man and his work. In the man a certain impatience of criticism ; and in his work, a consequent irritation when faced with the need for explicit formulation either of the methods used or of the assumptions tacitly made. To give an example of what is meant, De Morgan points out that the genius of Newton did not shine in the invention of mathematical language or symbolism. Fluxions apart, he says, " he added nothing to it. The notation of the *Principia* is anything but a model." † But De Morgan is too just and too careful a writer not to notice that there is another side to this, and one more flattering to Newton's genius. " We are avowedly expressing, in one point," he says, " our low estimate of Newton's power : and we believe the reason to have been, that he did not cultivate a crop for which he had no use. He who can make existing language serve his purpose never invents more : and Newton was able to think clearly and powerfully without much addition to the language he found in use. The *Principia,* obscure as it is, was all light in Newton's mind ; and he did not attempt to conquer difficulties which he never knew." ‡ The defect thus throws into greater relief the astonishing power of the man. We lose nothing of our sense of his mastery. Indeed a thinker of our own time, knowing the easier analytical modes of treatment, can express his admiration in fuller measure. " In reading his solutions," writes Mr. J. W. N. Sullivan, " one is struck by an inexplicable severity and formidableness. . . . He is like some immensely large, powerful, *lean* creature. Those immense swift strides, with a kind of noiseless, shattering force ! " §

---

* Cf. Halley's " nec fas est propius mortali attingere divos," at the end of his verses on the *Principia.*

† A. De Morgan, *Essays on the Life and Work of Newton,* p. 32.

‡ A. De Morgan, *loc. cit.* pp. 33-34.

§ J. W. N. Sullivan, *The Adelphi,* i. 12, p. 1107.

At the same time we gain an appreciation of how this conjunction of power and defective formulation could cover up the real implications of his work and impose upon succeeding generations a background of uncritically accepted ideas.

II.

It is precisely upon this creation, in Newton's time, of a solid but unobtrusive background of guiding ideas that we shall dwell. For it is in building what Prof. Whitehead has called this " first physical synthesis " that the true greatness of Newton's influence in physical science lies. Here is something infinitely more fruitful than the brilliant isolated results won by his supreme combination of mathematical and physical attainments. It is true the final synthesis was only made possible by the labours of many besides Newton ; in particular, Galileo. Indeed Prof. Whitehead goes so far as to say that you cannot disentangle their work. " There would have been no Newton without Galileo." * " Galileo represents the assault and Newton the victory." † " Think for a moment," he says, " of the possible course of history supposing that the life's work of these two men were absent. At the commencement of the eighteenth century many curious and baffling facts of physical science would have been observed, vaguely connected by detached and obscure hypotheses. But in the absence of a clear physical synthesis, with its overwhelming success in the solution of problems which from the most remote antiquity had excited attention, the motive for the next advance would have been absent. All epochs pass, and the scientific ferment of the seventeenth century would have died down. Locke's philosophy would never have been written ; and Voltaire when he visited England would have carried back to France merely a story of expanding commerce and of the political rivalries between aristocratic factions." ‡ Newton himself was, of course, fully aware of his debt to others. With the modesty of genius he could say, " If I have seen farther, it is because I have stood on the shoulders of giants."

The important point for us to notice is, however, that this great synthesis (whereby the old interpretation of nature in terms of substance and essence, matter and form, quantity and quality was replaced by description in terms of space, time and mass) was uncritically effected in the work of Newton. It was implicit. This has been admirably brought out in Prof. E. A. Burtt's penetrating study on the foundations of modern science. The time was ripe for change, and—as might well be expected—the genius who was at hand to effect it wrought better than he knew. In his treatment of the new categories which were now becoming fundamental in men's thinking ; in his acceptance of the doctrine of primary and secondary qualities ; in his notion of the relation between the physical universe and human knowledge—in all these (as Prof. Burtt truly remarks) Newton was constituting himself a philosopher, in spite of himself. Imbedded in the *Principia* " these metaphysical notions were carried wherever his scientific influence penetrated, and borrowed a possibly unjustified certainty from the clear demonstrability of the gravitational theorems to which they were appended as *Scholia*. . . . It may have helped not a little to insinuate a set of uncritically accepted ideas about the world into the common intellectual background of the modern man. What Newton did not distinguish, others were not apt carefully to analyse." § It is no disparagement of Newton's part in this great change, which altered so radically the presuppositions and imaginative content of men's minds, to say that we find close-knit and effective usage of the new conceptions without anything approaching explicit formulation of what was assumed and implied in them. It would be unreasonable to expect it. The right categories for the purpose in view were selected with marvellous skill and prophetic insight. It is (to use a phrase of De Morgan's) as though Newton hunted rather by scent than sight.

---

* A. N. Whitehead, "The First Physical Synthesis," in *Science and Civilization* (ed. Marvin), p. 174.

† A. N. Whitehead, *ibid.* p. 163.

‡ A. N. Whitehead, *ibid.* p. 162.

§ E. A. Burtt, *The Metaphysical Foundations of Modern Science*, pp. 20-21 : see also p. 226-227.

Nevertheless, in our task of estimating Newton's influence on the development of physical science, we have to observe that the immense success of Newtonian dynamics made the suppositions upon which it was based seem so natural that they were scarcely realized as assumptions at all. Prof. Whitehead has described the intellectual life of the two centuries which followed as a period in which the European races have been " living upon the accumulated capital of ideas provided for them by the genius of the seventeenth century." * In short, critical reconstruction was rendered inordinately difficult by the immense prestige of the synthesis, even when serious gaps were exposed in the structure—as in Berkeley's criticism of spatial ideas in his *New Theory of Vision*, and in Hume's attack on the inductive principle. What alone could finally arouse men of science from their dogmatic slumbers was the need, arising in physical science itself, for deeper foundations capable of carrying the expanding superstructure.

III.

When we turn from these more general considerations to the details of Newton's method we find the same implicit character. There is, on the whole, a practically achieved but scarcely rationally defined, empiricism. I imagine that Newton's most familiar methodological dictum is his " *hypotheses non fingo.*" The view suggested by this needs, obviously, a good deal of qualification in the light of Newton's other observations in various places and of his *Regulae Philosophandi.* Prof. Burtt has shown, indeed, that there was a considerable development in his attitude. Newton began his polemic against hypotheses in the mild form of declaring for their postponement until accurate experimental laws are established by a study of the available facts. Later " he felt himself forced to the conviction that the only safe method was to ban hypotheses entirely from experimental philosophy, confining himself rigorously to the discovered and exactly verifiable properties and laws alone." † The famous form of Newton's statement of this view occurs at the end of the *Principia* : " Hitherto we have explained the phenomena of the heavens and of our sea by the power of gravity, . . . But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses. For whatever is not deduced from phenomena, is to be called a hypothesis ; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction." ‡ It is clear from this that no place whatever is assigned, in experimental philosophy, to hypothesis. Newton no longer admits (as he once was inclined to do) that experiment and observation themselves are often guided by a bold guess. It is a straight denial, in fact, of the value of Leonardo's " Hypothesis is the general, and experiments are the soldiers." How did Newton come to adopt this extreme view ?

Now here we have, it seems to me, another instance of how the personal characteristics of the man influenced the form his views took : an instance, furthermore, which enables us the better to understand one of the sources of his power. Some of his early references to hypotheses were called forth by the criticism of Hooke on his paper to the Royal Society, soon after his election as Fellow, announcing his discoveries on the decomposition of light. Hooke had spoken of his suggestion that rays of light are corporeal as a hypothesis.

---

* A. N. Whitehead, *Science and the Modern World*, p. 58.

† A. E. Burtt, *loc. cit.* p. 213.

‡ *Principia*, 1713. p. 483.

"It is true," Newton replies, "that from my theory I argue the corporeity of light, but I do it without any absolute positiveness, as the word *perhaps* intimates, and make it at most but a very plausible consequence of the doctrine, and not a fundamental supposition. . . . Had I intended any such hypothesis, I should somewhere have explained it." * "I do not think it needful to explicate my doctrine by any hypothesis at all." † This early discussion brought out what De Morgan calls "that remarkable trait in his character"—an extreme aversion to any kind of opposition.‡ His annoyance was patent. "I intend," he said, "to be no further solicitous about matters of philosophy." This attitude grew on him to such a degree that there seems to be some justification for De Morgan's assertion that a morbid fear of opposition from others ruled his whole life. With this in mind it is easy to see how his view of the place of hypothesis (so naturally the subject of dispute) could become coloured by it.

And yet, when we look closer into the matter, it becomes evident that the drift towards a more thorough-going empiricism was just what was required at this period in the history of the physical sciences. Prof. Whitehead has described the scientific movement of this time as "a return to the contemplation of brute fact." It was based on a "recoil from the inflexible rationality of mediaeval thought." § This reaction was needed before the "union of passionate interest in the detailed facts with equal devotion to abstract generalisation which forms the novelty in our present society" could arise. We need only recall the title of Newton's main work—*The Mathematical Principles of Natural Philosophy*—to remind ourselves of the fundamental place which abstract generalisation takes in his view. And it may well be that his more or less irrational mistrust of hypothesis helped to keep the balance of his work on the empirical and experimental side truly adjusted to the needs he had to serve. It is easy enough for us, at this distance, to observe that an unrelieved empiricism is not merely unfruitful but literally unattainable. In the narrower fields of scientific work there is often (in Galileo's apt phrase) a "rape of reason upon the senses." And in the wider domains of scientific systems we can now see clearly enough that there is, in each age, and in Newton's no less than our own, a pervasive set of ideals of explanation which help to determine the form which our explanations take. But it must not be forgotten that our very recognition of this depends on the great first synthesis which made it possible for us to speak of conceptual "systems" at all in science.

There is one further aspect of Newton's dislike of dispute which should be mentioned as bearing upon the influence of his work on future ages. De Morgan lays considerable emphasis on the fact that "however much he might dislike or fear opposition, there was one tribute to it which his philosophy never paid; the pages he would gladly have burned rather than encourage discussion, contain no concession whatever." || We can be sure, therefore, De Morgan continues, that "Whatever Newton's opinions were, they were in the highest degree the result of a love of truth, and of a cautious and deliberate search after it. His very infirmity is a guarantee for the existence of this feeling in no usual measure. With a competent livelihood, and the dread of discussion so strong that he would gladly have hidden his results from the world rather than encounter even respectful opposition, he could not have worked either for the hope of wealth or office, or even for the love of fame, except in a very secondary degree. The enthusiasm which supported him through the years of patient thought out of which the *Principia* arose, must have been strong indeed when he had no ultimate worldly end to propose to himself. Who can say how much of the truth of his system we may owe to this very position?" ¶

---

[*] *Opera* (ed. Samuel Horsley), iv. 324.

[†] *Loc. cit.* iv. 328.

[‡] A. De Morgan, *loc. cit.* p. 17.

[§] A. N. Whitehead, *loc. cit.* p.12.

[||] A. De Morgan, *loc. cit.* p. 62.

[¶] A. De Morgan, *loc. cit.* p. 61.

PLAGIARISM IN THE SEVENTEENTH CENTURY, AND LEIBNIZ.

By Prof. L. J. Russell, M.A.

In 1646, Torricelli, writing to Cavalieri about one of his discoveries, expressed his fear lest knowledge of it might reach the French; for he was afraid that they might claim it as theirs, either as it stood or in developed form, and publish it first.* He did actually announce his discovery himself to the French mathematicians, among others Fermat, later in the same year; and of his partial communication to Mersenne he received acknowledgment; but hearing no word from the mathematicians themselves, he took the precaution, in 1647, of depositing a copy of his work with Michelangelo Ricci. On a previous occasion, he wrote, when he had told the French of his expression for the centre of gravity of the cycloid, they had first admitted the discovery was his, then doubted its truth and asked for a proof; but when he gave this, they remained silent for two years, and then claimed priority in its discovery. And he was afraid of a similar issue in this case.

The whole transaction, according to Bortolotti, was of no avail in his own day, nor for years after; for we find Fermat in 1657, in a letter to the English Digby, claiming the discovery as his own, and asserting that he had been in communication with Torricelli in regard to it.

We are not concerned here with the merits of the question. Our concern is to note that this is no isolated case. Descartes in his *Géométrie* deliberately left obscurities (so he said) in order that he might not be involved in disputes about priority; Hooke claimed to have anticipated Newton's account of planetary motion; Leibniz accused Tschirnhaus of publishing as his own many things which Leibniz had communicated to him; finally, there is the celebrated charge of plagiarism from Newton brought against Leibniz himself. And Leibniz's latest English defender, Mr. Child,† while freeing him from the charge of stealing from Newton, finds himself compelled to suspect Leibniz of having been indebted to Barrow to a much greater extent than Leibniz was willing to confess, and of carefully avoiding anything which might lead to a disclosure of this fact.

Other results, of an indirect nature, are connected with all this. It is well known that in many cases mathematicians concealed their results; or communicated results without communicating methods; or set problems to their contemporaries, the solution of which depended on knowledge of certain methods; or communicated problems in anagrams, or even in jumbles of letters. Sometimes in answer to a problem, a mathematician would set another problem which, he said, was equivalent to the problem set.

What, now, are we to say to all this? What are the conditions in which such secrecy and such mutual distrust were possible?

I think that there were two principal causes operating in the period from 1600 to 1660 or 1670, and that the habits then formed led to a situation which it was not easy to alter in the period from 1670 to 1700.

---

* Ettore Bortolotti, *Archivio di Storia della Scienza*, Sept. 1924.

† J. M. Child, *The Early Mathematical Manuscripts of Leibniz*, Open Court, 1920.

The first cause lies in the state of mathematics itself. The field of mathematics was like a district in which gold has been discovered at various places. You might at any moment hit on a simple substitution, *e.g.* in an algebraic equation, or in a summation of a series, that would lead on to a new general method; or on a new comparison of similar triangles in a geometrical figure, that would lead to the solution of one of the well-known, but unsolved, problems. Sometimes even the hint that someone had discovered a method for solving a particular problem was enough to set you looking in the right direction for solving it, and you could solve it too.

In such a situation, what is needed is a clearing house of publicity. And here we come to the second cause. There was before 1660 no method of publishing save either by the writing of books, or by the writing of letters. Neither method is appropriate for the kind of communication which finds its natural place in the *Proc. Roy. Soc.* or in the *Mathematical Gazette*; the method of publication by means of letters had its unsavoury side; and the various scientific journals seem to have arisen in response to the need for a more public means of communication.

The Accademia del Cimento, founded at Florence in 1657; the Royal Society, founded in 1660; and the Académie des Sciences, founded in 1666, gave official stamp to the communications of their members. The *Philosophical Transactions* was issued in 1664; the *Journal des Savants*, in 1665; these being followed, in Italy, by the *Giornale dè' Letterati*, in 1668. Thereafter there are a number of journals, of which the best known are the *Acta Eruditorum* of Leipzig, founded in 1682; the *Nouvelles de la République des Lettres*, in 1684; the *Histoire des Ouvrages des Savants*, in 1687.

But mathematicians still went on to some extent communicating by means of letters.

Under such general conditions, then, the mathematicians of the seventeenth century worked; and there were bound to be charges of plagiarism brought, when such charges were unfounded.

Leibniz has been cleared of the charge of stealing his calculus from Newton, by the publication of fragments from his papers preserved in Hanover; but there still hangs about him something sinister.

Child sees that he cannot have been guilty of the common charge, but seems to suggest that he was indebted to Barrow's *Lectiones Geometricae* more than he originally realised, and that then, when he did realise this, he found himself in a false position, from which he could extricate himself only by a careful suppression of the facts relating to his study of Barrow.

As a result of such an investigation Leibniz comes to be regarded as not entirely unscrupulous, but yet as not wholly honourable; and from that it is easy to look on him, as some of the historians of mathematics still do, as capable of deliberate lying and falsification of evidence.

The more of Leibniz's writings I have read, the more I have been convinced of the unsoundness of such charges. The main criticisms of Child are due to the imperfect publication of Leibniz's papers. When Leibniz said that he found, not in Barrow but in Pascal, the theorem leading to his early thoughts on quadratures, and that after he had made his discoveries he found them later in Barrow, Child, not finding in Pascal the diagram corresponding to what Leibniz said, but on the contrary finding in Barrow both the theorem which Leibniz said he discovered and a diagram to which what Leibniz said could be referred, and knowing that Leibniz actually possessed a copy of Barrow, decided that Leibniz was at any rate not speaking accurately; and from that he went on to ask why. But a further examination of Leibniz's unpublished papers, reported by Mahnke,* has shown that the early work of which Leibniz spoke was concerned with Pascal and not with Barrow, thus vindicating Leibniz on this point. For the notes which Leibniz made when he was reading Pascal's *Traité des Sinus* are still among his papers, and they show his gropings toward his more general method. It becomes clear of course that when Leibniz in his old age says, " I saw at a glance, . . . " the real process was much less rapid ; but his indebtedness to Pascal is set beyond doubt.

---

* Dietrich Mahnke, "Abhandlungen der Preussischen Akademie der Wissenschaften," 1925, *Phys.-Math. Klasse*, No. 1.

Incidentally Mahnke shows what a rich harvest still remains to be gathered from the unpublished writings of Leibniz ; and it would seem that meanwhile it would be better not to set our conjectures against Leibniz's assertions in such a way as to involve him in charges of dishonesty.

THE CONTEMPORARY SIGNIFICANCE OF NEWTON'S METAPHYSICS.

By Prof. E. A. Burtt, Ph.D.

If it may be laid down as a general principle that the profitable time to study a great man's thought is after he has lost his position of quasi-supernatural authority, surely now is the time for intensive research into the main ideas of Newton. And since it is his metaphysics which has been discarded rather than his mathematical advances or experimental discoveries in the various branches of physics (for these, strictly speaking, once made, are never lost), it would seem that perhaps the most fruitful type of enquiry would be one which approached Newton from the standpoint of interest in the metaphysical problem of modern thought and aimed to clarify his significance for contemporary reconstruction in metaphysics.

Of course Newton's conscious reaction to metaphysics was one of vigorous opposition, as to a collection of quite unverifiable "hypotheses," but since no one can avoid ultimate assumptions of some sort he was, like most scientists, a metaphysician against his will. For that very reason it is easier to see just where the heart of his difficulties really lay than in the case of more consciously sophisticated metaphysicians, in whose case all our energy is exhausted in finding out what they really meant or (in case they did not know themselves) what they probably would have meant had they been able to know.

The central problem of metaphysics in modern times may be formulated thus: *If we take the empirical point of view as seriously as it deserves, in what sense is it possible to work out a science of the ultimate structure of the universe?* For, amid many variations of conception, metaphysics has always meant some such ultimate and inclusive science. To be sure, no metaphysician, so far as I know, has stated the problem in just these terms (though Kant's abstruse phraseology is not far from this in its purport), but it is what in point of fact the situation reduces to when we get our larger bearings.

Now what has actually happened in the work of self-confessed metaphysicians in modern times is that sooner or later they have renounced empiricism, embracing some set of principles or other as being absolute and beyond the possibility of change. The only variation is in the set of principles thus chosen as final and in the reasons assigned for believing them eternally fixed. In this they have but followed the first systematic metaphysician, Aristotle, whose ultimate principles seem to have been derived from Greek grammar, and who naïvely assumed that thought must always and everywhere follow the rules of syntax of his native language. The way in which this metaphysical renunciation of empiricism has taken place in modern times is well illustrated by the procedure of Kant. Kant was familiar enough with empirical science to realize perfectly well that if what we know is dictated by the empirical object, there can be nothing absolute in it, for the object's self-revelation is never final. But the distinction between logical thought and empirical fact, which he inherited from his metaphysical and scientific predecessors, and whose implications he had attempted to follow up in his early essays, came to his rescue. Thus the Copernican revolution, as he describes it, comes exactly to this: if the object must in certain ways conform to thought, rather than thought in all ways conform to the object, then metaphysics is possible; we can chart the ultimate structure of thought and pronounce objects of experience necessarily endowed with the qualities thus implied. What, now, if this structure of logical thought itself prove not so absolute or unchangeable as the scheme presupposes? Kant seems never to have considered this possibility; he lived at a time when the main rules of logic had come down practically untransformed from the days of Aristotle, and found himself able, by a little dexterous manipulation, to correlate them with the central assumptions of mathematico-experimental science, which also had remained almost constant from the time of Galileo. The last hundred and fifty years have of course witnessed the complete undermining of his foundations. The old logic has been purged of error and subsumed under still more general deductive principles; and the concepts of space, time, causality, and the like, which furnished the foundations of science for Newton and Kant, have all been abandoned, at least in their traditional form, and science is still wandering in the dark in search of dependable successors. In consequence of this revolution men of empirical loyalty have been forced away from the rationalism that underlay the quest for metaphysics all along the line, the tendency becoming more and more outspoken to deny the possibility of metaphysics outright. This has been hastened by the resurgence of cosmic evolutionism in our day and its pact of alliance with empirical science; all things whatsoever, according to this viewpoint, are subject to change, growth, and decay, including the most general categories as well as the most transitory events. The outcome is of course that metaphysics comes to seem quite incompatible with faithfulness to empirical fact. There appears to be nothing in the universe protected from change; there *is* no eternal structure of reality, from whence it follows at once that there is no science of it.

But alas for our attempt to rest content with this result! As we examine critically the works of these champions of the ubiquity of change we are disturbed by unmistakable signs of metaphysics, which lead us to suspect that there was something in the old rationalistic postulate after all. For if everything changes, then in the concept of change, at least, we have an ultimate metaphysical category which can be confidently affirmed of the universe, and all the consequences involved in the meaning of change can be developed by deductive logic and also applied with assurance to reality. This is most clearly evident in a philosophy such as M. Bergson's, but it is as truly discoverable in any systematic defence of an ultimately empirical and practical point of view if we are willing to penetrate to the assumptions that form its core. We see accordingly that the metaphysical problem has not been evaded after all, and if we continue to reflect about the matter we sooner or later come back to some such formulation of it as has above been suggested: granted the validity, in the main, of the empirical point of view, in what sense is it possible to work out a science of the ultimate structure of the universe?

Here let us turn to Newton, and see what help we can derive from him. In his pages, as in those of most scientists, the issue concerns, on the one hand, (1) the relation between the empirical and the mathematical assumptions in science, and (2) the relation between the scientific categories and those of extra-scientific beliefs, such as religion, on the other. On the first point Newton's famous *Rules of Reasoning in Philosophy* express his position better than any other statement. The necessary rationalistic assumption of all thought, which opens the door to metaphysics if it be granted, is stated after Newton's fashion in the first rule.* "We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances." That is, we must assume that appearances form an ordered, intelligible system, and we must not assume anything more. The second rule is really a corollary of this: " therefore to the same natural effects we must, as far as possible, assign the same causes." The third and fourth rules raise our problem in further detail and attempt to answer it more fully. The third seems to extend our rationalistic assumptions beyond the limits assigned by the first—" The qualities of bodies, which admit neither intension nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments [Newton illustrates by extension, mobility, gravity, etc.], are to be esteemed the universal qualities of all bodies whatsoever "—while the fourth asserts clearly an ultimate empiricism in the form: " In experimental philosophy we are to look upon propositions collected by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions." Just what is the relation between these two rules ? Our answer is given in his comment on the third rule. " We are certainly not to relinquish the evidence of experiments for the sake of dreams and vain fictions of our own devising; nor are we to recede from the analogy of Nature, which uses to be simple, and always consonant to itself." In the light of Newton's entire discussion I would venture to summarize his meaning as follows: The testimony of experiment is the ultimate criterion of truth, but experiment itself is impossible unless we assume that Nature is an intelligible order, that is, that its baffling complexity can be reduced to the simplicity of law. This assumption is therefore final and absolute. Furthermore, as a methodological principle (not absolute) we have the right to presume that bodies yet to be studied have the same qualities as have been universally revealed in those already experienced, but this is subject to the proviso that further experience confirm the presumption.

---

* *Principia*, Motte translation, 1803 ed., vol. ii. p. 160 f.

It is noticeable that the universal qualities Newton is interested in here are those in virtue of which bodies become mathematically treatable. Are such the *only* universal qualities of bodies, and are they to be assumed present in *every* reality, corporeal or not ? We are now approaching the second point, and the answer, given mainly in the *General Scholium* to the *Principia* and the later *Queries* appended to the *Optics*, is a decided negative to both questions. The very quality of intelligible order itself, which points toward divine authorship, and the esthetic and moral effects which flow from it, are not quantitative in nature; and there are for Newton incorporeal realities, such as God and the soul, whose behaviour is quite beyond the reach of mathematical formulation.

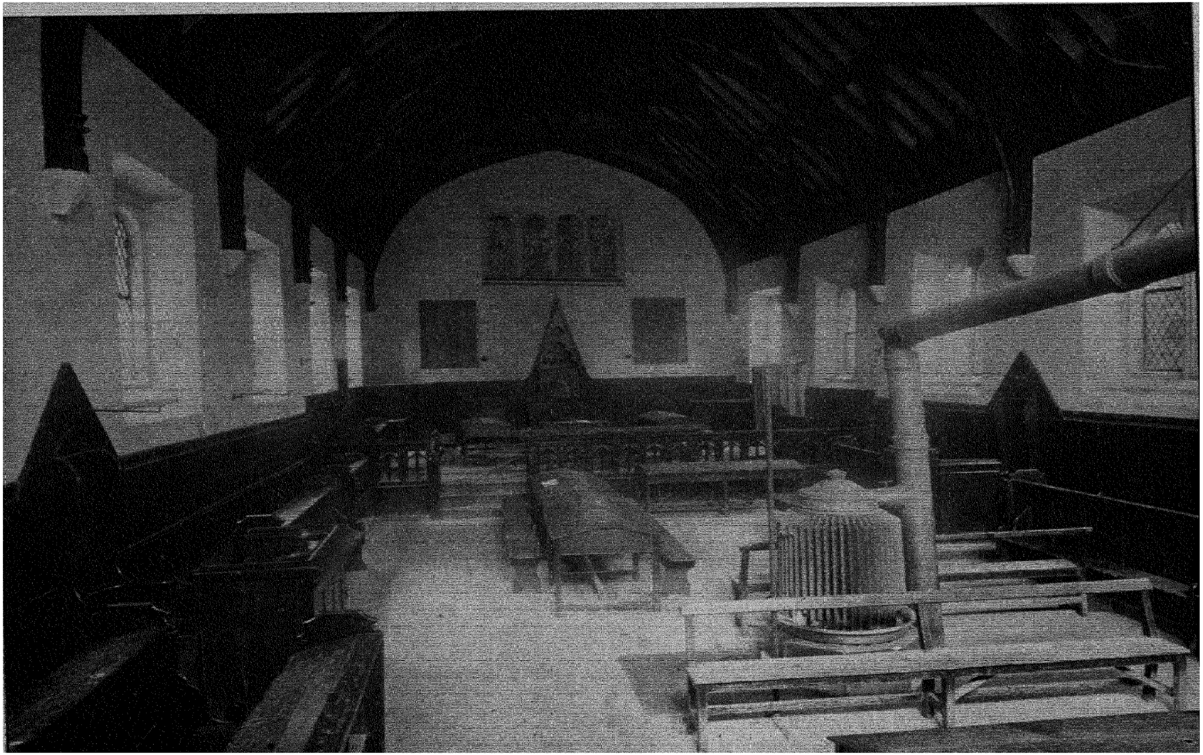
Now of course Newton did not develop this situation as a systematic metaphysician would try to do, but it seems to me that it is full of capital suggestions as to what the task of metaphysics really is from a sanely empirical point of view, especially in the light of the historical adventures of Newton's leading ideas. Is it not apparent, not merely that there is one category which for every thinker must be absolutely final and unchangeable (that of intelligible order as above expressed), but also that other categories vary, so to speak, in the degree of their proximity to it; those nearest partaking most of its stability, those farther away more liable to change by the gnawing tooth of time. This may be illustrated by any category you choose, simply by following up its relations from this point of view.

Suppose we take the category of space, meaning by this term, to begin with, the space of Euclidean geometry. It is already obvious from scientific history that as soon as mathematics develops the notion of non-Euclidean spaces, and physicists discover facts that resist construction in terms of Euclidean space, the latter category tends to be supplanted by another (that of space in general), of which the Euclidean is one species, constructed by adding further postulates. Such considerations yield a principle in accordance with which we may proceed farther in the same direction; the more specific postulates we shear away from the meaning of any category, the more general the explanatory purpose represented by the resulting category, and accordingly the less liable to change it will be. Advancing by this principle we may say that space in general is in turn more liable to change or abandonment than the more general character of extensive quantity, since we are more likely to give up geometry as a tool than to change in our fundamental necessity of dealing somehow in quantitative ways with that which possesses extension. In turn, extensive quantity is more liable to change than quantity in general, for even though we experienced the highly unlikely revolution of finding ourselves no longer interested in measuring the extended, we might still think in mathematical terms with reference to intensive quantities. And finally the category of quantity would sooner change than the category of intelligible order or continuity. For the former to be upset our interest in securing exact explanations would have to cease in favour of some alternative inconsistent with it—a possibility so remote as to seem practically impossible—while the latter category is absolute and unchangeable as a character of what presents itself as the world to anybody who chooses to think. For it to change we should have had to cease thinking entirely, and therefore our world as an experienced object would no longer exist. And we know nothing beyond experience.

Metaphysics thus becomes the science which reveals the nature and place of the intermediate categories between the absolute category reflected by the postulate which all thinkers have to make and the categories of the other sciences. But it is essential to observe that the allocation cannot be made merely by observing the relations of implication between the categories recognized by the sciences themselves (as contemporary rationalists are prone to suppose), for apart from the functioning of a live explanatory interest and a recognition of the ultimacy of the category it involves, the hierarchy will remain very incomplete. It will tend to culminate in the highly abstract postulates of mathematics, thus neglecting the existence of non-quantitative logical relations and the real explanatory interests they reflect.

Except with respect to its one absolute postulate, then, metaphysics will be a changeable and growing science like every other, but it will less rapidly change than any other, inasmuch as its function is precisely to bridge the gap between the most general categories of other branches of thought and the ultimate postulate of all thought. Since the latter is final and unchangeable, the science which affirms it and is, so to speak, nearest to it, will be the most stable science. The part of the wheel nearest the hub moves less rapidly than the rim.

This is, of course, to restrict metaphysics to ontology, or the ultimate theory of being. There is another type of enquiry which might, if one wishes, be included in metaphysics, namely that which arises from the fact that the significance of the absolute category itself grows as one continues to expand his experience and clarify his thought. But its basic meaning as logical continuity remains unchangeable through this enlargement, and that fact makes possible a science of metaphysics as above stated.



The Interior of the Old School, The King's School, Grantham.

NEWTON AND HIS HOMELAND—THE HAUNTS OF HIS YOUTH.

BY J. A. HOLDEN, M.A., The King's School, Grantham.

"Nature and Nature's laws lay hid in night;
God said, *Let Newton be!*—and all was light."

POPE'S well-known couplet brings to a focus the intensity of the pride felt by contemporaries in the genius of Newton. But the corner of England which produced this intellectual giant holds interest for all—magic, indeed, for some.

Seven miles south of Grantham and a few hundred yards west of the Great North Road lies the tiny hamlet of Woolsthorpe-by-Colsterworth, consisting of two or three small farmhouses, a few cottages, and behind—farthest of all from the road—Newton's birthplace. The Manor House is a large building of grey stone, facing west and overlooking the orchard which contains a scion of the famous apple tree. The front of the house is covered with ivy. Over the front door, which is small and very low, is a stone tablet which reads: "In this Manor House Sir Isaac Newton was born, 25th December, 1642." This is surmounted by two shin bones saltier-wise. Inside are large rooms with very low ceilings and old stone floors, even in the bedrooms—deep-set windows and stone fireplaces. Upstairs, the room in which Newton was born is on the left. Pope's couplet is inscribed on a stone table over the fireplace; there is an old oak cupboard with carved panels set in the wall, and a drawing of the celebrated apple tree. The room on the right of the staircase is supposed to have been Newton's bedroom. A space in the south-east corner is partitioned off and is pointed out as Newton's study. Two windows, now blocked up, are included, one facing east, the other south. Several windows in the house were built up at the time of the window-tax, and though some have since been restored, this probably explains the fact that there are extant several different photographs of the house.

Most of Newton's boyhood was spent at Woolsthorpe. His father had died before the boy's birth, and two years later his mother married again and went to live at North Witham. Newton remained at Woolsthorpe in the care of his grandmother, Mrs. Ayscough, and as a small child attended schools in the neighbouring hamlets of Skillington and Stoke.

At the age of twelve Newton was sent to the King's School, Grantham. The old school building in which he received his education dates back to Tudor times, and, though now dismantled, is still in regular use as an assembly hall. Until the nineteenth century—when Church Street was made—the school opened directly on to the churchyard, which did duty as the boys' playground. Newton tells us himself that at first he did not shine at school, and held a very low place in class. One day he received a kick from a boy whose place was higher than his, and challenged him to fight. Newton won, and, instigated by young Stokes, the headmaster's son, proceeded to rub his opponent's nose on the wall of the church. This victory seems to have encouraged him to beat his foe in lessons as well; he soon reached the top of his class, and remained there from that time onwards.

On the whole, Newton seems to have been a healthy, normal and by no means perfect boy. He evidently indulged in that represensible habit of carving one's name in a public building, for among a crowd of names on one of the window-ledges in the old school appears that of I. NEWTON.

Newton did not take a great part in the games of his companions. During term-time he boarded in the High Street " at the next house to the George Inn, northward," with an apothecary named Clarke, whose brother was mathematical master at the school. Here he was able to indulge to his heart's content his inclination for making mechanical models. At the age of nine he had carved with his penknife a sundial out of a block of stone. This dial he fixed with his own hands on the south wall of Woolsthorpe Manor House, but it was removed about fifty years ago to Colsterworth Church, and was built into the wall behind the organ.

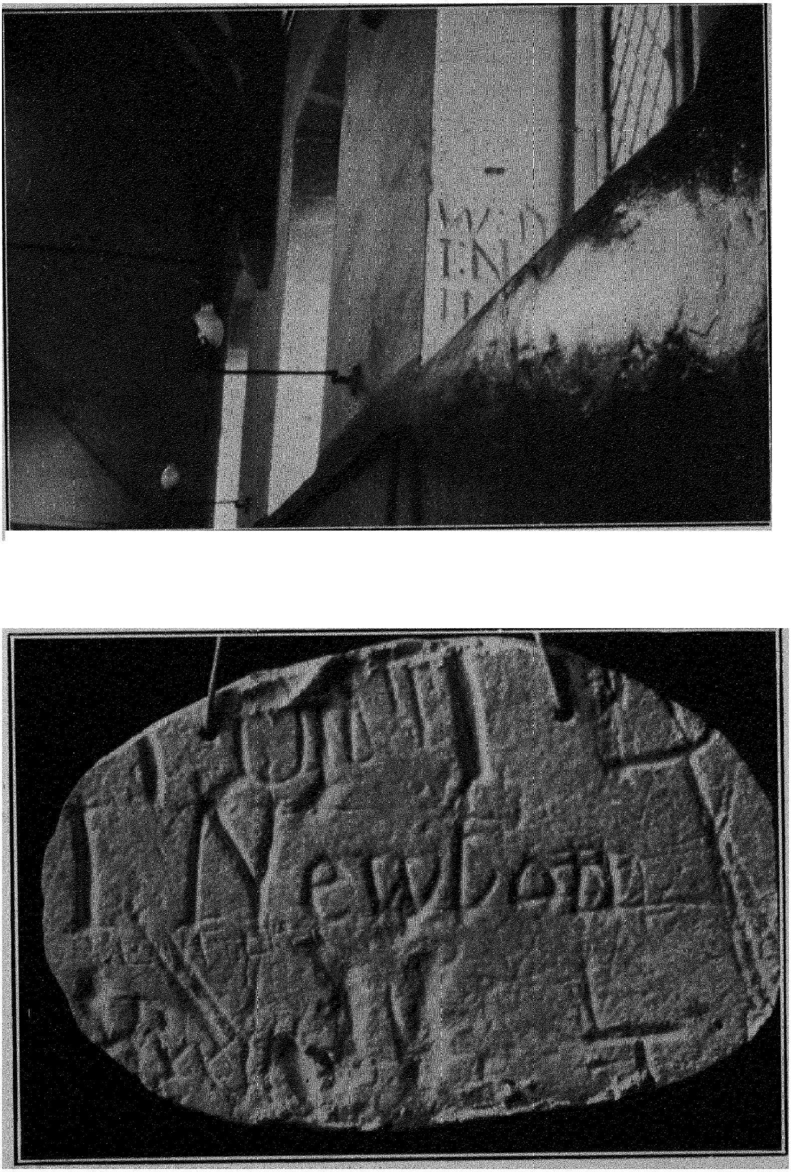
At Clarke's house Newton's leisure hours were fully occupied. Perhaps the contents of the apothecary's bottles helped to awaken the taste for experiment. He made a water-clock out of a box which he begged from Mrs. Clarke's brother. When finished, it stood four feet high and was of proportionate breadth. The index of the dial-plate was turned by a piece of wood which rose and fell by the action of the dropping water. This clock stood in his own bedroom, and he himself kept it supplied with water. It was used as a clock by the Clarke family, and remained in the house long after Newton had left Grantham. Another model he made was that of a windmill. For several days he had watched some workmen constructing a windmill near Gonerby. He made an excellent model for himself, and would often fix it on the roof of Clarke's house, so that the wind could put the sails in motion. He also tried to develop the idea of driving it by animal power, and used to catch mice to place inside it. Another of his " inventions " was a paper lantern, by the light of which he went to school in winter, and which he sometimes attached to the tail of a kite to frighten the superstitious.

When Newton was fifteen years of age his stepfather died. His mother returned to Woolsthorpe and recalled her son from school, for she wanted him to be a farmer and to keep up the yeoman traditions of his father's family. As Newton showed no inclination for farming, his mother very wisely consulted her brother, William Ayscough, rector of the next parish, Burton Coggles. He realised that his nephew's mind was wholly set upon mathematical and mechanical problems, and through his advice Newton was sent back to Grantham School, with the definite object of preparing for a university career at his uncle's old college—Trinity College, Cambridge.

He stayed at Grantham until he was eighteen, pursuing the usual course of classics, and when the day came for him to leave the school, the headmaster, Henry Stokes, " with the pride of a father, placed his favourite pupil in the most conspicuous part of the school, and having, with tears in his eyes, made a speech in praise of his character and talents, held him up to the scholars as a proper object of their love and admiration."

During his stay at the apothecary's house in Grantham, Newton seems to have formed an attachment for a Miss Storey, a " young and blooming " girl, and a niece of Mrs. Clarke. His affection for her seems to have made a deep impression on Newton's heart. He preferred her society to that of his schoolfellows, and used to spend a good deal of his time making little tables or cupboards for his lady. This attachment presumably continued while Newton was at Cambridge, but marriage would have caused him to forfeit his fellowship, and, as he had no means to support a wife, he apparently consoled himself with study.

To one so truly great, one cannot ascribe absent-mindedness, but many stories are told of his complete concentration when absorbed in some deep problem. He was once riding home from Grantham, and dismounted to let his horse walk up a steep hill. At the top he turned to mount again, only to find



By the courtesy of the Grantham Museum Committee.

CARVED INSCRIPTIONS IN THE KING'S SCHOOL, GRANTHAM.

that his horse had slipped away, leaving the bridle in his hand. On another occasion, a friend, Dr. Stukeley, called on him. Newton was out, but the table was laid for dinner. Dr. Stukeley lifted the cover, ate the dinner, and then replaced the cover. When Newton appeared later he greeted Stukeley, and, sitting down, lifted the cover—" Dear me," said he, " I thought I had not dined, but I see I have."

In the Grantham Museum there is a drawing representing Newton, apparently in interested conversation with an attractive young lady, but the whole effect is spoiled—from the lady's point of view—when the great man uses her finger to press down the tobacco in his pipe.

For Newton his birthplace and the home of his youth had a special appeal. He returned thither when driven from Cambridge by the plague, and we frequently read of his return to duty after periods of quiet spent in Lincolnshire. The special characteristics of the country-born were his. The " call " of the country was in his blood—he must have felt that " call " when he wandered down to Stourbridge Fair at the beginning of his first term at the university. His shyness, his indifference to praise, his great humility can probably all be traced to the influence of that quiet home in the country, where, far from pomp and circumstance, men can view life not, as in towns, by artificial standards, but according to eternal truths. Newton himself wrote: " I see not what there is desirable in public esteem, were I able to acquire and maintain it: it would perhaps increase my acquaintance, the thing which I chiefly study to decline."

TRINITY COLLEGE IN THE TIME OF NEWTON.

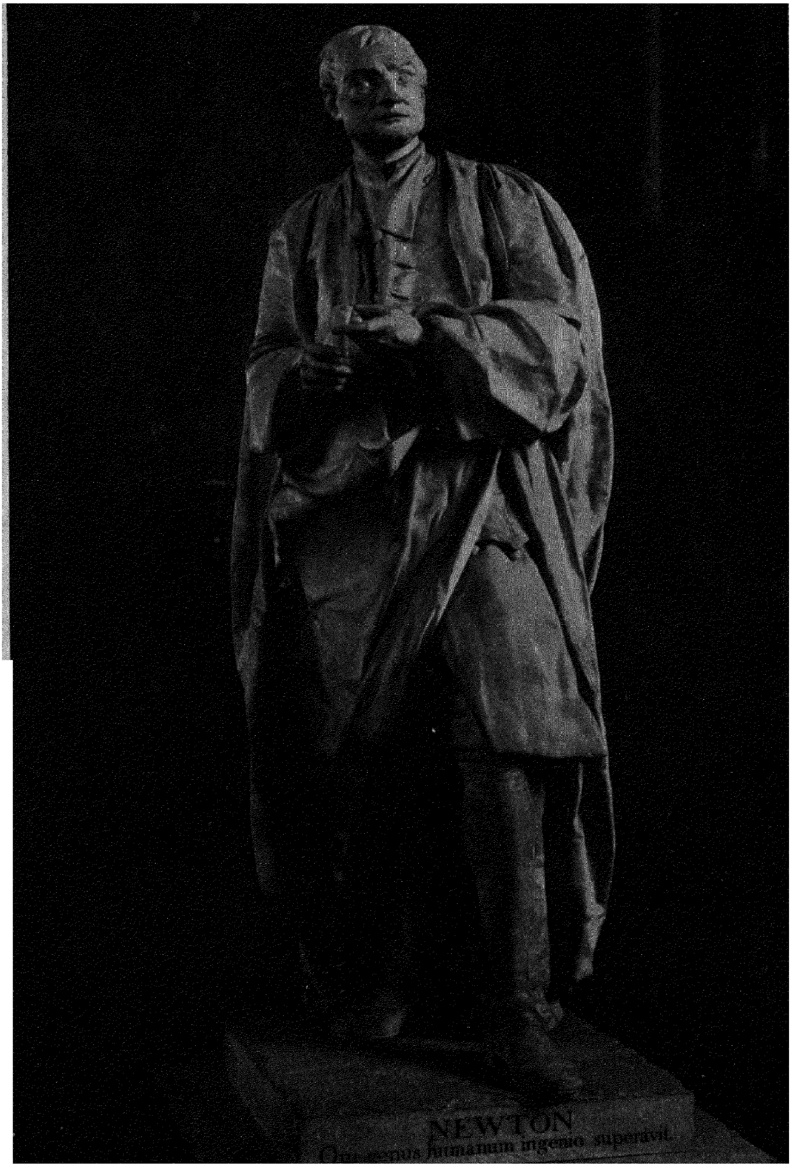
BY PROF. G. N. WATSON, SC.D., F.R.S.

WHEN I received the request to contribute to the *Gazette* an article on some aspect of Newton's life, it seemed obvious that I ought to select a topic about which I had some knowledge; this consideration ruled out at once everything to do with his discoveries in Applied Mathematics; and, after taking thought, it did not appear to me that I could produce anything particularly illuminating either on the subject of his discovery of the Differential Calculus or on his later work in the *Analysis Infinitorum.* It consequently seemed like an inspiration when it occurred to me to give some slight account of the buildings and society in which Newton lived from 1661 till the end of the seventeenth century. Had the occasion arisen for me to attempt such a theme more than a couple of years ago, I should not have dared to do so, since it could have been done so much more adequately by the late Mr. Rouse Ball. Those who are acquainted with his works and with the works of Mr. J. W. Clark will at once realise how much this sketch owes to their researches.

It is probably safe to assume that the majority of the readers of an English mathematical journal have at least some knowledge of Cambridge, and so for the most part it will be possible to describe the buildings of the college as they were two and a half centuries ago by enumerating their similarities with and their differences from the buildings as they stand to-day.

With the extant materials which are readily accessible, it is not very difficult for a Cambridge man to visualise the general appearance of the College. David Loggan's *Cantabrigia Illustrata* was published in 1690, and though these engravings are now becoming somewhat scarce (one of my most prized possessions is the engraving of Nevile's court in Trinity College), they were republished on a somewhat reduced scale in 1905 by J. W. Clark with a dedication to that most venerated of Trinity men of our day, Henry Jackson. Loggan's set of engravings includes three views of Trinity College; and his accompanying Survey of Cambridge together with Fuller's bird's-eye view of the town, in his *History of Cambridge* (1634), both of which are reproduced in *Old Plans of Cambridge* (1921)—a project of which Clark did not live to see the publication—give a vivid idea of the environment of the College.

The construction of the Great Court of Trinity College had been completed with the building of the Hall in 1605; throughout Newton's time it presented the appearance shown by Loggan in his general view of the College. The chief subsequent alterations have been the erection of an observatory on the top of the Great Gate in the first decade of the eighteenth century, and this, after being neglected for at least half a century, was removed in 1797; the rebuilding of the south-west corner of the court in 1770, this involving the demolition of the old Hall of Michaelhouse with its beautiful oriel window; and alterations to the windows of the Master's Lodge in 1700 by Bentley, and again in 1843 by Whewell. The stucco on the north and east sides of the court was not applied till 1750. And so, with the exception of the distant south-west corner, which was partially hidden, then as now, by the fountain



By the courtesy of Mr. John Murray.

Roubiliac's Statue in Trinity.

in the centre of the court, the view of the Great Court from the rooms in the north-east corner, between the Great Gate and the Chapel, which Newton occupied during his tenure of his Fellowship, was much the same as it is to-day.

It is not quite so easy to imagine the aspect of the College from Trinity Street, then called Trumpington Street. Loggan's general view of the College shows gardens laid out between the College and the street, with a high wall round the garden on Newton's side of the Great Gate; but both Loggan's and Fuller's plans seem to indicate buildings, as at present, between the College and the street on the south side of the Great Gate. Probably the gardens existed, but Loggan's view gives a slightly misleading impression as to their width, so that there was room for a row of houses between them and the street. Minor features of Loggan's view which must be mentioned are the fences round the grass-plots in the court (so that, in Newton's time, Fellows of the College could not have walked across the grass in comfort), two dogs fighting or playing between staircase H and the Hall,* and the eagle (shown as being about to attack a third dog), which must have been a contemporary of Newton, for the College Accounts show that a perch and a chain were bought for it in 1684, and a tub in 1685.

But, while no very extensive change has been made in the general appearance of the Great Court since before Newton's time, the situation with regard to Nevile's Court is very different. The buildings erected by Thomas Nevile in the early part of the seventeenth century consisted of two wings stretching westward from the buildings on the west side of the Great Court, of length two-thirds (or, more accurately, three-fifths) of the cloisters as they stand to-day. The west ends of these wings were joined by a wall, in the middle of which was Nevile's gate leading to a tennis court. This gate, when the wall was pulled down, was removed to the end of the Avenue, and thence to Trinity Lane, to a site some yards south of where it stands to-day.

In the earlier part of Newton's time at Cambridge, the College Library was housed in the "Old Library" in the north-west corner of the Great Court; this was subsequently converted into chambers which many Trinity men will remember as being occupied for a long period by the late Senior Fellow, Mr. Joseph Prior. The building of the "New Library," standing on massive pillars and forming the west side of Nevile's Court, was begun in February 1676, the architect being Sir Christopher Wren,† and it was completed by 1690. There were consequently gaps between the west ends of the wings built by Nevile and the ends of the Library. These gaps were filled by prolonging the cloisters, the chambers above them being provided, on the west half of the north gap in 1676-9 by Sir Thomas Sclater, formerly Fellow, on the east half of the north gap "by the benefaccion of many worthy persons," and on the east half of the south gap in 1681-2 by Dr. Humphrey Babington, Senior Fellow; the west half of the south gap was also completed at about the same time. The north cloister contains a remarkable echo, and there is a tradition in the College that Newton made use of it to determine the velocity of sound. In more recent times repetitions of his experiments by undergraduates were apt to be interrupted by the appearance of Aldis Wright, the Vice-Master, who occupied the chambers built by Sclater, and who had not much sympathy with such disturbances of his studies.

It would appear so far from this brief account that Nevile's Court in Newton's later days was the court which we know to-day; but this is not so; the original chambers above both cloisters were buildings of an elaborate Jacobean style, and, being badly in need of repair by 1750-1760, were rebuilt, to match the Library, in the severely classical style with which we are familiar.

---

* The rules against dogs apparently date from the early nineteenth century. Had the rules existed by 1808, they would probably have been invoked against Lord Byron's bear.

† An account of an alternative design by Wren, which provided a large circular building for the library, forms Chapter IX. of Rouse Ball's *Cambridge Notes*.

The remaining College chambers formed the Bishop's Hostel, a short distance to the south of the west side of the Great Court. Funds for the building of this, on the site of the ruins of Gerard's (or Garret) Hostel, were provided in August 1669 by Dr. John Hacket, Bishop of Lichfield and Coventry, sometime Scholar and Fellow; he gave £1200 for that purpose to the "Right Reuerend and most worthy Gouernors of that Societie which is more precious to mee, next to the Church of J. Xt, then anie place upon Earth." To judge from Loggan's engraving, this building has undergone no substantial change since 1670.

Other college buildings were the stables, brewhouse, etc., and Loggan's general plan also shows an L-shaped block of buildings near the site now occupied by the south-west corner of the New Court. And finally the chief natural adornment of the College must be mentioned, namely the avenue of lime trees, planted in 1671-2 on the open ground to the south-west of the College, which has lasted till our own day, though several of the trees may not survive many more winters.

After this description of the buildings of the College, and of the changes which were effected in them during Newton's residence, it is opportune to say a few words about the members of the Society which occupied the House. It was a much smaller Society than it is to-day; in Newton's first year, forty-one freshmen matriculated, as compared with the modern couple of hundred. The number of members on the Foundation has not, however, undergone a similar increase; there were sixty Fellows and forty Scholars on the Foundation of Henry VIII., twenty Scholars on the Foundation of Queen Mary, in addition to the Westminster scholars, a few scholars on other foundations, and sixteen sizars; and these numbers are of much the same order of magnitude as the corresponding numbers of to-day. Unfortunately the College Admission Books do not record parentage, age and school until 1676, so it is not possible to glean from them the previous history of Newton's two score contemporaries, except that eight came from Westminster. Newton entered as a subsizar in 1661, was elected a Scholar in 1664 and Fellow in 1667; he proceeded B.A. in 1664-5 and M.A. in 1668. The Ordo Senioritatis (the forerunner of the Tripos list) for 1664-5 is unfortunately lost, but the *Cambridge Historical Register* records that the examiners in that year were John Eachard of St. Catharine's and Benjamin Pulleyn of Trinity, and that, besides Newton, John Strype, the ecclesiastical historian, took his degree in 1664-5.

The Masters of the College in Newton's time were John Pearson (1662-1673), previously Master of Jesus College, who was also Lady Margaret's Professor of Divinity and subsequently Bishop of Chester; Isaac Barrow (1673-1677), Newton's predecessor in, and first holder of, the Lucasian chair of Mathematics; John North (1677-1683), who was also Regius Professor of Greek; John Montagu (1683-1700); and in 1700, Richard Bentley (1700-1742), formerly of St. John's, in his own phrase ". . . leaped over a wall" (Ps. xviii. 29), the beginning of his stormy reign at Trinity almost synchronizing with the appointment of Newton as Master of the Mint. Among the Vice-Masters of the period, the names of James Duport (Dean of Peterborough and subsequently Master of Magdalene) and Humphrey Babington are still annually recited in the College Roll of Benefactors.

Newton's Tutor was Mr. Pulleyn, previously mentioned, a Leicestershire man, who was subsequently Professor of Greek, the senior of his pupil by eleven years; but it is recorded that Newton learnt much of his mathematics while an undergraduate from Barrow. And Newton is himself described as Tutor in the years 1669 and 1680-1687; it should be explained that, until the middle of the eighteenth century, many, if not most, of the resident Fellows acted as Tutors, some having only one or two pupils. In those days a Tutor, in addition to being *in loco parentis* to his pupils, was charged with the task of instructing them; to quote the Statutes of 1560 which were in force till

1844: "Pupilli Tutoribus pareant, honoremque paternum ac reverentiam deferant, quorum studium labor et diligentia in illis ad pietatem et scientiam informandis ponitur. Tutores sedulo quae docenda sunt doceant: quaeque etiam agenda instruant admoneantque." A casual inspection of the Admission Books reveals but two pupils of Newton, one in 1669 and one in 1680. Both were Fellow Commoners, and neither graduated; his other pupils, if any, must have been transferred to him from other Tutors after admission.

Newton, however, must have taken a more lively interest in College tuition than these facts would indicate, for he has left a most elaborate manuscript scheme for tuition, though this scheme seems never to have got beyond the stage of private discussion; it has been printed by Rouse Ball as Chapter XX. of his *Cambridge Notes.* The scheme provides for tuition in Humanity, Greek, Philosophy and Mathematics by Lecturers, and in Logic, Ethics, the Globes and principles of Geography and Chronology by the Tutor. A most interesting part of the scheme is the last paragraph: "No oaths of office to be imposed on the Lecturers. I do not know a greater abuse of religion than that sort of oaths. . . ."

A few of Newton's contemporaries at Trinity who attained high office in the University may be named here, in addition to those already mentioned: George Villiers, Duke of Buckingham (Zimri), was Chancellor, 1671-1674; Charles Seymour, 'the proud' Duke of Somerset, was Chancellor, 1689-1748; it is narrated of him that his second wife ventured to kiss him on their wedding-day, and was crushed by his rebuke "Madam, my first wife never dared to take such a liberty, and she was a Percy"; Charles Montagu, Earl of Manchester, was High Steward; William Ayloffe was Public Orator; Purbeck Richardson was Esquire Bedell; in March 1683 he "in a fit of insanity destroyed himself with a musket in his chamber in Trinity College"; James Valentine, Robert Creighton, Thomas Gale (later High Master of St. Paul's School) and Michael Payne were Regius Professors of Greek; the curious may find in one of Rouse Ball's works an entertaining story of an accusation of profanity being made against Creighton at the time when he was sitting for the Fellowship Examination; Wolfran Stubbe, James Talbot and Henry Sike were Regius Professors of Hebrew; Charles Wright was Professor of Arabic; John Colbatch was Professor of Moral Philosophy; Roger Cotes was Plumian Professor; James Manfield and John Laughton were Librarians of the College and of the University. Other eminent contemporaries of Newton, selected from the list in Cooper's *Memorials of Cambridge,* were Abraham Cowley, the poet; Charles Chauncey, president of Harvard College; Charles Frasier, physician to Charles II.; William Perrey, Professor of Music at Gresham College; Roger Meredith, Professor of Law at Gresham College; these were all Fellows, and so we may be certain that they were numbered among Newton's acquaintances.

Had this sketch aimed at being a biography of Newton's whole life, it would not have been inappropriate to end it with a quotation from an old play: "Newton! Oh ay—I have heard of Sir Isaac—everybody has heard of Sir Isaac—great man—master of the mint." But since it has dealt merely with the times in which he resided in Trinity College, it is more fitting to conclude with the noble inscription on the nobler statue by Roubiliac in the Ante-Chapel: *Newton qui genus humanum ingenio superavit.*

A NEWTON BIBLIOGRAPHY.

By H. Zeitlinger.

In the history of Science no one can be credited with as many and as important discoveries, with as large a number of works, and with as long a list of commentators and antagonists as Newton. G. J. Gray's *Newton Bibliography* gives 412 entries, nor do these exhaust all that is known.

A concise and systematic list of his works would therefore not seem out of place. The alterations which the editions underwent, and their additions, have moreover another interest. They throw a vivid light on Newton's character, gentle, but shy and extremely reluctant to enter into controversies, with which he was fated to be overwhelmed with remorseless insistence, often on trivial grounds, and sometimes by persons whose only claim to be now remembered rests on the fact that they once opposed Newton.

OPTICAL RESEARCHES.

Newton's earliest printed work was connected with his optical researches, and consisted of sixteen papers, all of which were published in the *Philosophical Transactions of the Royal Society*, between February, 1672, and September, 1676.

The most important of these was the first, which was published in No. 80, of February 19th, 167½ (pp. 3075-87) as *A Letter of Mr. Isaac Newton, Professor of the Mathematicks in the University of Cambridge, containing his New Theory about Light and Colors; sent by the Author to the Publisher from Cambridge, February 6th,* 167½ ; *in order to be communicated to the Royal Society.* " The Contents " preceding the title gives it as " containing his New Theory about Light and Colors: Where Light is declared to be not Similar or Homogeneal, but consisting of difform rays, some of which are more refrangible than others: And Colors are affirm'd to be not Qualifications of Light, deriv'd from Refractions of natural Bodies (as 'tis generally believed;) but Original and Connate properties, which in divers rays are divers: Where several Observations and Experiments are alleged to prove the said Theory."

It gives the results of his experiments with the glass prism, which he had bought at Stourbridge Fair in 1661, much of the work having been done before 1665, when he had to leave Cambridge on account of the Plague. They contain the important discovery, only remotely guessed at before him, that solar light is not homogeneous, but consists of seven primary colours, each of which has a different refrangibility, and can no further be resolved. From these he infers chromatic aberration, unknown before (although spherical aberration was well known), which led him to consider that the reflecting telescope was the only one which could overcome this difficulty.

He gives an explanation of the colours of thin plates, shows how to recompose white light by means of a second prism, distinguishes between primary and compound colours, and fully explains the theory of the primary and secondary rainbow—only partially done before him by Archbishop Marc' Antonio de' Dominis, in a work published in 1611—and explains the colour phenomena of

" lignum nephriticum, leaf gold, fragments of coloured glass, and some other transparently coloured bodies."

" Colours are not quantifications of light, derived, from refractions, or reflections of natural bodies, (as 'tis generally believed,) but original and connate properties, which in divers rays are divers " (p. 3081). " To the same degree of refrangibility ever belongs the same colour, and to the same colour ever belongs the same degree of refrangibility " (p. 3081). He declines to form a theory of the nature of light, when he says : " But, to determine more absolutely what light is, after what manner refracted, and by what modes of action it produceth in our minds the Phantasms of Colours, is not so easie. And I shall not mingle conjecture with certainties " (p. 3085).

The importance of these discoveries was immediately recognised. The solemn thanks of the meeting of the Royal Society were voted to its author for his " very ingenious discourse," and the paper was ordered to be printed immediately for " securing the considerable notices thereof to the author against the arrogations of others." These expectations proved only too true, but alas ! in a sense quite different from that anticipated by the Royal Society.

The second paper was published in No. 81 of March, 1672, as (2) *An Accompt of a New Catadioptrical Telescope invented by Mr. Newton* (pp. 4004-10), and gives an account of his first reflecting telescope, which he had made in 1668. There were several suggestions of such a telescope before this date, the earliest being by James Gregory in his *Optica Promota* (1663), but Newton's was the first successful reflector ever actually made.

In the same year also appeared (3) *Mr. Newton's Letter . . . containing some more Suggestions about his New Telescope, and a Table of Apertures and Charges for the several Lengths and Charges of that Instrument* (No. 82, pp. 4032-34), to which was appended (4) *An Extract of another Letter of the same . . . by way of Answer to some Objections, made by an Ingenious French Philosopher to the New Reflecting Telescope* (pp. 4034-35). This contains further constructional details, and points out the relation between focal length, aperture and magnification. The objector was Ignace Gaston Pardies, S.J.

In May of the same year appeared (5) *Considerations upon part of a Letter of Monsieur de Bercé . . . concerning the Catadrioptrical* [*sic*] *Telescope, pretended to be improved and refined by M. Cassegrain* (No. 83, pp. 4056-59). After a translation of the essential part of de Bercé's letter, which originally appeared in the *Mémoires de l'Académie Royale des Sciences,* Newton mentions that Cassegrain had " about three months communicated to me a figure of a telescope, which was almost like it [Newton's] . . . but which I look upon as more witty " (p. 4056). He mentions that the idea of Cassegrain's telescope was contained in James Gregory's *Optica Promota* (1663), and goes on to say : " I had had thence an occasion of considering that sort of constructions, and found their disadvantages so great, that I saw it necessary, before I attempted any thing in the Practique, to alter the design of them, and place the Eye glass at the side of the Tube rather than in the middle " (p. 4057). After enumerating the disadvantages of Cassegrain's telescope, he says : " By this you may perceive, that the three advantages, which Monsieur Cassegrain propounds to himself, are rather disadvantages " (p. 4058), and concludes by saying : " I could wish therefore, Mr. Cassegrain had tried his design before he divulged it " (p. 4059). Incidentally Newton mentions the interesting fact that " about 6 or 7 years since Mr. Gregory himself, at London, caused one [reflecting telescope] of six foot to be made by Mr. Reive, which I take to have been according to the aforesaid design described in his book ; because, though made by a skilful Artist, yet it was without success " (p. 4059).

In the same month was published another paper, (6) *Some Experiments propos'd in relation to Mr. Newton's Theory of Light . . . together with Observations made by the Author of that Theory* (No. 83, pp. 4059-62). This discusses four experiments with the prism, " made by some friend at Cambridge."

Newton's optical discoveries were of such importance, and so much in conflict with the prevailing theories that they could hardly fail to bring forth a host of objectors. The first of these was Father Pardies, whose objection to the telescope had already been answered in Newton's fourth paper.

Some objections of the former raised in connexion with the prism experiments produced (7) *Mr. Newton's Letter . . . being an Answer to the fore-going letter of P. Pardies*, which appeared in No. 84 (pp. 4091-3). It is written in Latin.

Arising out of his controversies with foreign critics, he published in June, 1672, (8) *A Series of Quaere's propounded by Mr. Isaac Newton, to be determin'd by Experiments, positively and directly concluding his new Theory of Light and Colours* (No. 84, pp. 5004-7), in which he wishes "that all objections were suspended, taken from *Hypotheses*, or any other heads than these two; of shewing the insufficiency of Experiments to determine these *Quaere's* or prove any other part of my Theory, by assigning the flaws and defects of my conclusions drawn from them; or of producing other Experiments which directly contradict me, if any such may seem to occur" (p. 4005).

The next paper, (9) *Mr. Newton's Answer to the foregoing letter* (No. 84, pp. 5014-18), which is written in Latin, gives Newton's final reply to Father Pardies, who had to confess in a postscript to it that "omnino mihi satisfecit novissima responsio, à Dn. Newtono ad meas instantias data."

A more formidable antagonist arose in Robert Hooke, who attempted to show that Newton's discoveries had been anticipated by himself, and that Newton's experiments proved his own hypotheses, as advanced in his *Micrographia* (1665). Newton's answer is contained in an important paper, published in November, 1672, (10) *Mr. Isaac Newton's Answer to some Considerations upon his Doctrine of Light and Colors: which Doctrine was printed in Number* 80 *of these Tracts* (No. 88, pp. 5084-5103).

Here he first outlines his corpuscular theory: "Assuming the Rays of Light to be small bodies, emitted every way from shining bodies, those when they impinge on any Refracting or Reflecting superficies must as necessarily excite vibrations in the *æther*, as Stones do in water when thrown into it" (p. 5087), without, however, definitely adopting it, as appears from a further sentence: "'Tis true, that from my Theory I argue the *Corporeity* of Light; but I do so without any absolute positiveness, as the word *perhaps* intimates; and make it at most but a very plausible *consequence* of the Doctrine, and not a fundamental *Supposition*" (p. 5086), and again: ". . . I do not think it needful to explicate my Doctrine by any *Hypothesis* at all" (p. 5091).

He answers Hooke, who tried to fasten him down to the undulatory theory, by saying that he had spoken of light, "in *general* terms, considering it abstractly as something or other propagated every way in streight lines from luminous bodies, without determining, what that thing is" (p. 5087), and ". . . it . . . seemed impossible . . . that the *Waves* or Vibrations of any Fluid, can, like the Rays of Light, be propagated in *Streight* lines, without a continual and very extravagant spreading and bending every way into the quiescent Medium, where they are terminated by it" (p. 5089).

A reply to Christiaan Huygens, the propounder of the undulatory theory of light, who had objected to Newton's theory of white light, and the different refrangibility of the different colours, produced (11) *Mr. Newton's Answer to the foregoing Letter further explaining his Theory of Light and Colors, and particularly that of Whiteness: together with his continued hopes of perfecting Telescopes by Reflections rather than Refractions* (No. 96, pp. 6087-92), and (12) *An Extract of Mr. Isaac Newton's Letter . . . concerning the Number of Colors, and the Necessity of mixing them all for the production of White: as also touching the Cause why a Picture cast by Glasses into a darkened room appears so distinct notwithstanding its Irregular refraction* (No. 97, pp. 6108-11), which first describes his Colour Disk.

Some trivial objections by Franciscus Linus, S.J., were answered in the next two papers, published between January and March, 1676: (13) *Mr. Isaac Newton's Considerations on the former Reply : together with further Directions, how to make the Experiments controverted aright* (No. 121, pp. 500-2) ; (14) *An Extract of another Letter of Mr. Newton . . . relating to the same Argument* (No. 121, pp. 503-4). Of more importance were the objections of Gascoine, Linus's pupil, and of Antonius Lucas, who were answered in the following two papers: (15) *A particular Answer of Mr. Isaac Newton to Mr. Linus his Letter . . . about an experiment relating to the New Doctrine of Light and Colours* (No. 123, pp. 556-61), and (16) *Mr. Newton's Answer to the precedent Letter* [*of Lucas*] (No. 128 (1676), pp. 698-705).

Brewster remarks in his *Life of Newton* that " the experiments of Lucas and Newton on the length of the spectrum have a close connexion with the different dispersive powers of bodies, which was one of the greatest discoveries of the following century, and led to the invention of the achromatic telescope."

The effect of these persistent attacks on a character naturally so shy and sensitive as Newton's, who was always very unwilling to be drawn into controversies, may be well imagined. He states them himself in a letter to Leibniz : " I was so persecuted with discussions arising from the publication of my theory of light, that I blamed my own imprudence for parting with so substantial a blessing as my quiet, to run after a shadow." Henceforward he showed himself very unwilling to make public any of his discoveries. Yet, as fate would have it, this very circumstance involved him in another controversy infinitely more virulent—that with Leibniz and his followers.

Henry Oldenburg, the Secretary of the Royal Society, cannot be quite absolved from blame in this connexion. Being neither a prolific writer nor a discoverer, he could feel little sympathy for an author deeply immersed in following up the trend of his discoveries, and rather encouraged controversies with foreign savants, *ad majorem Societatis Regiæ gloriam*, no doubt.

Five more papers were published in the *Philosophical Transactions*, the first three anonymously. (17) *Epistola missa ad praenobilem virum D. Carolum Mountague* [*sic*] . . . *in qua solvuntur duo problemata Mathematica à Johanne Bernoullo Mathematico proposita* (No. 224 (1697), pp. 384-9). This contains the solution of two celebrated problems, one on the brachistochrone, proposed by Johann Bernoulli and Leibniz in the *Acta Eruditorum*. Newton solved them in one day, although six months were allowed for their solution.

The next paper, (18) *Scala graduum Caloris. Calorum Descriptiones & signa* (No. 270 (1701), pp. 824-9), describes his thermometer, made with linseed oil, which had the temperature of melting snow and that of the human body as bases. It throws an interesting light on the accuracy of Newton's observations, for he did not adopt the boiling temperature of water as basis, because he found that it varied. He was, however, unable to connect this variation with barometric pressure.

This paper is important for containing the law of the cooling and heating of bodies—modified in 1818 by Dulong and Petit—that the quantity of heat lost or gained by a body in a second is proportional to the difference between its temperature and that of the surrounding medium ; but it does not express it in the well-known differential equation known by his name.

Another paper, (19) *A General Solution of Curves, formerly proposed in the Leipsic Acts* (1716), contains the solution of the problem to determine the curve which should cut at right angles an infinity of curves of a given nature, but expressible by the same equation, and was his last mathematical effort.

(20) *A true Copy of a Paper, in the handwriting of Sir Isaac Newton, found among the Papers of the late Dr. Halley, containing a Description of an Instrument for observing the Moon's Distance from the Fixed Stars at Sea* (vol. xlii, p. 155), was published in 1743, and contains his invention in 1700 of the reflecting quadrant. It was published in answer to Hadley, who claimed the invention in 1731. Brewster's *Life*, where a full description of the instrument

will be found in vol. i, pp. 240-2, mentions that "either from having mislaid the manuscript, or from attaching no value to the invention, he never communicated it to the Royal Society." Another paper on chronology is noticed in its place, *passim.*

These papers were the only ones Newton ever contributed to the publications of the Royal or any other learned Society, and but for the reprints in the *Abridgements of the Philosophical Transactions* by Lowthorp and Hutton, were never reprinted.

In 1704 appeared his *Opticks*: *or, a Treatise of the Reflexions, Refractions, Inflexions and Colours of Light. Also Two Treatises of the Species and Magnitude of Curvilinear Figures.* It was the only one of his works which Newton ever prepared for the press himself, and contains, systematically arranged, the substance of his papers in the *Philosophical Transactions.* Here he definitely adopts his corpuscular or emission theory of light, and complements it by the hypothesis of fits of easy reflexion and transmission, to explain the inflexion (diffraction) of light. There are also new researches on the diffraction of light, including an explanation of the colours of thick plates, and at the end fifteen queries. Of special interest are the chemical, as these, besides his paper *De Natura Acidorum,* are the only chemical researches he ever published. The former include a definition of fire and flame, and one query on elective attractions, giving his view on the composition of bodies : "the smallest particles of matter may cohere by the strongest attractions, and compose bigger particles of weaker virtue, and many of these may cohere and compose bigger particles whose virtue is still weaker." The mathematical treatises appended will be mentioned in their proper place. There is also an undated issue of this edition without them. It was probably the one seen by Brewster, as he states that the work was published without a date.

A second edition came out in 1717 in octavo, the first being in quarto size. It has a new preface, while the number of queries is increased from 16 to 31, which include the celebrated (No. 28): "Are not all hypotheses erroneous in which Light is supposed to consist in pression or motion propagated through a fluid medium ? . . . If it consisted in pression or in motion propagated either in an instant or in time, it would bend into the Shadow. For pression and motion cannot be propagated in a fluid in right lines beyond an obstacle which stops part of the motion, but will bend and spread every way in the quiescent medium which lies beyond the shadow." Query 17 discusses the double refraction of Iceland spar.

In advertisement II he makes an interesting statement as to the cause of gravitation : "At the end of the 3rd book I have added some Questions. And to shew that I do not take Gravity for an essential Property of Bodies, I have added one Question concerning its Cause, chusing to propose it by way of a Question, because I am not yet satisfied about it for want of Experiments." Neither this nor any later edition gives the mathematical treatises.

A second issue of the second edition, which is far more common than the first, was published in 1718.

A third edition appeared in 1721, and is mainly a reprint of the second. The fourth edition, which was the last revised by Newton, and contains only minor corrections, was published in 1730.

The work was translated into Latin by Samuel Clarke, D.D., who received for his work £500 from Newton "as a token of the approbation and gratitude of the author." It appeared in 1706, in quarto size, as *Optice, sive de Reflexionibus, Refractionibus, Inflexionibus et Coloribus Lucis Libri III, Latine reddidit Samuel Clarke, A.M. Accedunt Tractatus II ejusdem Authoris de Speciebus et Magnitudine Figurarum Curvilinearum.* It is noteworthy for first containing Newton's views on Iceland spar, for having six more queries than the English edition of 1704, and for first giving that on the nature of light, which appeared as No. 28 in the second edition of 1717.

A second edition of this translation was published in 1719, but without the nathematical treatises. The queries in it are extended from 22 to 31, the ast being "Annon exiguæ corporum particulæ certas habent virtutes, potenias, sive vires, quibus per interjectum aliquot intervallum, agant, non modo in adios luminis, et eos reflectendos, refringendos, et inflectendos," etc., etc., etc., illing pp. 381-415.

Continental reprints of the Latin edition were published at Lausanne, 1740, ith a copious index (absent in the English editions), another, undated, at raz, which includes also a translation of the *Optical Lectures* and the *Optical apers* published between 1672 and 1676, but in Latin translations, and a hird and fourth at Padua in 1749 and 1773.

Of other contemporary translations there were only those in French by ierre Coste, from the second English edition, of which two editions appeared, ne at Amsterdam in 1720, and the other at Paris in 1722. As the biographer f Newton in the *Dictionary of National Biography* remarks, the translation was of durable service, and helped to introduce English thought to the rench of the XVIII. Century." Another French translation was by Jean aul Marat, that monster of the French Revolution, which appeared in 1787. he translator depreciatingly calls Coste's translation "défectueuse, infidèle et bscure, servile et barbare." Marat's translation is of special interest, as it as he who first dared to attack Newton's corpuscular theory of light, then ll-powerful in France.

A German translation appeared as late as 1898, in Ostwald's *Klassiker.*

The *Lectiones Opticæ,* Annis MDCLXIX *ad* MDCLXXI, *in Scholis Publicis abitæ: et nunc primum ex MSS. in Lucem editæ,* were published postumously in 1729, from a copy given by Newton to David Gregory, Savilian rofessor at Oxford. They consist of the course of lectures Newton had elivered from his Lucasian chair, and are divided into two parts, "de adiorum lucis refractionibus" and "de colorum origine." According to rewster, they "contain many beautiful propositions, and interesting and nstructive experiments, which are not to be met with in any modern treatise n optics."

An English edition, but of the first part only, was published in 1728, one ear before the appearance of the Latin original. It contains an interesting reface by the anonymous translator.

It must be noted as a singular fact, as pointed out by Brewster, that Newton's eputation was not founded on his optical discoveries, but on his reflecting elescope, to which he was also indebted for his election as a Fellow of the oyal Society.

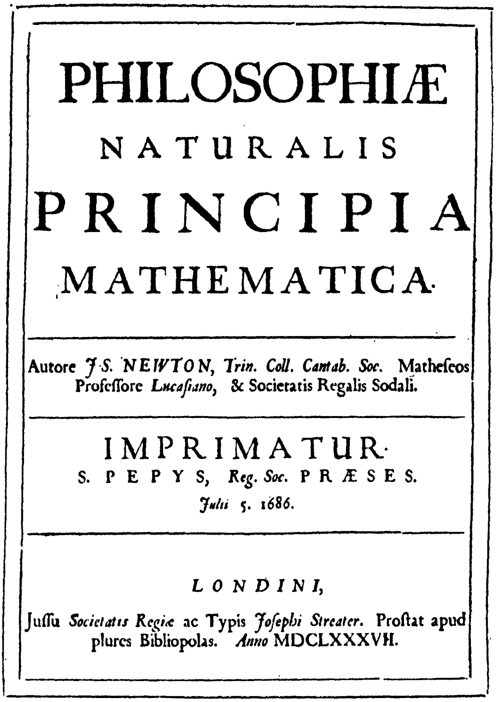
PRINCIPIA.

The year 1687 will for ever remain memorable in the annals of science for aving witnessed the birth of the *Principia,* the greatest work on exact science hat the human mind has ever conceived.

Its origin goes back to the year 1666, when Newton took up the question of ravitation. Pemberton relates that as Newton "sat alone in the garden [at Voolsthorpe] he fell into a speculation on the power of gravity," which he then lready connected with the lunar and planetary motions. Voltaire enlarged his account by adding the well-known incident of the falling apple that is said o have started the train of thought extending the force of gravitation beyond he apple into space and to the moon and the heavenly bodies.

These researches, however, were not completed, and it was not until 1684, r early in 1685, when in his treatise *De Motu* he first proved mathematically he law of universal gravitation, the outstanding discovery of the *Principia.* lthough the law of the inverse square of the distance, which was but the logical equence of Kepler's laws, was guessed at by many, in a more or less complete orm, it was Newton who first proved it, and extended it to the motions of the eavenly bodies.

This important and historically most interesting treatise *De Motu* was reprinted in Rigaud's *Historical Essay*, and in W. W. R. Ball's *Essay on Newton's Principia*. Enlarged and completed it formed the first book of the *Philosophiæ Naturalis Principia Mathematica*, which was published in the summer of 1687. Edmund Halley, to whom posterity is indebted for having influenced Newton to work out and complete the problem of gravitation, not only corrected the proofs, but bore the cost of printing. The work is divided into three books, the first two being *De Motu Corporum*, of which the first treats of the motions of particles in free space, and the second of their motions in a resisting medium, of hydrostatics, hydrodynamics, etc. The third is entitled *De Mundi Systemate*, and contains his astronomical discoveries. The work has a dedication to the Royal Society, a "Praefatio ad Lectorem," and Latin verses to Newton by Halley, but no Index.



PHILOSOPHIÆ
NATURALIS
PRINCIPIA
MATHEMATICA.

Autore *IS.* NEWTON, *Trin. Coll. Cantab. Soc.* Matheſeos
Profeſſore *Lucaſiano*, & Societatis Regalis Sodali.

IMPRIMATUR.
S. PEPYS, *Reg. Soc.* PRÆSES.
Julii 5. 1686.

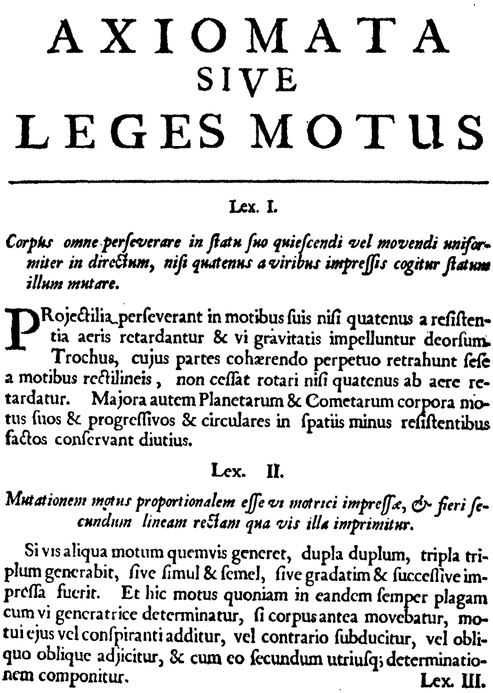
LONDINI,

Juſſu *Societatis Regiæ* ac Typis *Joſephi Streater*. Proſtat apud
plures Bibliopolas. *Anno* MDCLXXXVII.

Only the more important discoveries need here be mentioned. Besides its outstanding discovery, the law of universal gravitation, with its theory of attractions, the application of the principle of mechanics to the solar system, and the creation of physical astronomy, it formulates, in an introductory chapter, *Axiomata, sive Leges Motus*, the three laws which form the basis of the modern science of dynamics, creates the lunar theory, with the discovery of the moon's precession, the cometary theory, the theory of tides, which Newton first indisputably connected with the moon's attraction, and a theory of the propagation of waves, with its application to the determination of the velocity of sound.

The success of the work was immediate, but only a small number of copies having been printed, it soon became scarce. A copy of the work, says Brewster "could scarcely be procured in 1691. Cotes states in his preface to the second edition, that copies of the first were scarce and could only be obtained at an immense price. Sir William Browne, when at college, gave more than two guineas for a copy, and owing to the difficulty of procuring one at a reasonable price, the father of Dr. John Moore, of Glasgow, transcribed the whole work with his own hand."

A second issue of the first edition is known, which merely consists in having a new title with the addition in the imprint of "Prostant venales, apud Sam. Smith ad insignia Principis Walliae in Cœmiterio D. Pauli, aliosq. nonnullos Bibliopolas," and was evidently issued to satisfy the demands of the London booksellers.



AXIOMATA
SIVE
LEGES MOTUS

Lex. I.

Corpus omne perſeverare in ſtatu ſuo quieſcendi vel movendi uniformiter in directum, niſi quatenus a viribus impreſſis cogitur ſtatum illum mutare.

PRojectilia perſeverant in motibus ſuis niſi quatenus a reſiſtentia aeris retardantur & vi gravitatis impelluntur deorſum. Trochus, cujus partes cohærendo perpetuo retrahunt ſeſe a motibus rectilineis, non ceſſat rotari niſi quatenus ab aere retardatur. Majora autem Planetarum & Cometarum corpora motus ſuos & progreſſivos & circulares in ſpatiis minus reſiſtentibus factos conſervant diutius.

Lex. II.

Mutationem motus proportionalem eſſe vi motrici impreſſæ, & fieri ſecundum lineam rectam qua vis illa imprimitur.

Si vis aliqua motum quemvis generet, dupla duplum, tripla triplum generabit, ſive ſimul & ſemel, ſive gradatim & ſucceſſive impreſſa fuerit. Et hic motus quoniam in eandem ſemper plagam cum vi generatrice determinatur, ſi corpus antea movebatur, motui ejus vel conſpiranti additur, vel contrario ſubducitur, vel obliquo oblique adjicitur, & cum eo ſecundum utriuſq; determinationem componitur.

Lex. III.

After an interval of twenty-six years a second edition came out in 1713, at the urgent solicitations of Richard Bentley, Master of Trinity College, Cambridge. Its publication was entrusted to Roger Cotes, who added an important sixteen-page preface, in which he attacks the Cartesian philosophy then still in vogue at the universities, and refutes an assertion that Newton's theory of attraction is a *causa occulta*. It has the original preface by Newton with an addendum by him, dated March 28th, 1713, in which he points out the chief alterations, and Halley's verses, which were unwarrantably altered by Bentley, without the knowledge of Newton or Halley. An Index Capitum and an Index Rerum alphabeticus are here first added. About 750 copies were printed, of which 250 were sent to Holland and France.

The alterations in this edition were considerable, the principal being in Book II, propositions 34-40 (on the resistance of fluids), and in Book III, prop. 39 (on the precession of the equinoxes), and props. 41-2 (on the cometary theory). Many corollaries were added and others left out, and some propositions were partly rewritten.

A piratical reprint of the second edition was published at Amsterdam in 1714, and again in 1723, with the addition of *Analysis per Quantitatum Series.*

The third and last edition was published in March, 1726, exactly one year before Newton's death, under the editorship of Henry Pemberton. It is the only one that was reprinted. A "large paper" and a "largest paper" edition were also issued, the latter in less than twenty copies, all reserved for presentation.

It contains a new preface by Newton, mentioning some of the additions, the most important of which were a modification of the scholium on fluxions, and a new scholium to prop. 33 of Book III on the motions of the moon's nodes. Halley's verses were again restored to their original form, and a portrait of Newton, by G. Vertue, after a painting by Vanderbank, was added.

A reprint, incorrectly known as the "Jesuits' Edition," was published at Geneva in 1739-42 by Thomas Le Seur and François Jacquier, with a copious commentary, and the addition of three prize essays on the theory of tides by Daniel Bernoulli, Colin Maclaurin, and Leonhard Euler. This edition was again reprinted at Geneva in 1760, and, edited by J. M. F. Wright at Glasgow in 1822 and 1833, but without the three prize essays. Gray's *Bibliography* also mentions a reprint at Prague in 1780-5, with a commentary by J. Tessanek. A very correct reprint of the third edition, on good paper, and in a clear type, was published at Glasgow in 1874, under the editorship of Lord Kelvin and Hugh Blackburn.

Of abridgments there is *Excerpta quædam e Newtoni Principiis Philosophiæ Naturalis, cum Notis Variorum,* edited (anonymously) by John Jebb, George Wollaston, and Robert Thorp, and published at Cambridge in 1765, also Robert Thorp's *Excerpta,* from Le Seur and Jacquier's edition (Oxford, 1831), and Dr. Whewell's reprint of Book I, Sections 1-3, with Notes and References (London, 1846).

It might seem strange that only two contemporary translations were made of a work of such importance, were it not for the fact that the comparatively few who were capable of understanding it would all know Latin as the *lingua franca* of science during the seventeenth and the early part of the eighteenth century.

An English translation was published in 1729 (in two vols.) as *The Mathematical Principles of Natural Philosophy, translated by Andrew Motte, with the Laws of the Moon's Motion, according to Gravity, by John Machin, F.R.S.* It was reprinted (in three vols.) in 1803 and 1819, *with Newton's System of the World, a short Comment on, and Defence of, the Principia, by W. Emerson, and Life, carefully revised and corrected by William Davis.* There is also an American reprint of this edition, with a Life by N. W. Chittenden, of New York, 1848 and 1850. The principal addition to these editions was a translation of *De Mundi Systemate Liber Isaaci Newtoni. Opus diu integris suis partibus desideratum. In Usum Juventutis Academicæ,* first published at London in 1728, and reprinted in 1731, while an anonymous English translation appeared in 1728, 1731, and 1737. De Morgan (*Budget of Paradoxes*) doubted whether this work was by Newton, and added: "It is very possible that some observant turnpenny might construct such a treatise as this from the third book, that it might be ready for publication the moment Newton could not disown it," to which Prof. D. E. Smith, in his new edition of the *Budget,* appends a note saying that "It is no longer considered by scholars as the work of Newton." These statements do not seem to take into account a passage in the preface to the third book of the *Principia* of 1687, in which Newton expressly declared that he had originally composed this book in a popular manner, but had afterwards rewritten it: "De hoc argumento composueram Librum tertium methodo populari, ut à pluribus legeretur. Sed quibus Principia posita satis intellecta non fuerint, ii vim consequentiarum minimè percipient, neque præjudicia deponent quibus à multis retro annis insueverunt: & propterea ne res in disputationes trahatur, summam libri illius transtuli in Propositiones, more Mathematico, ut ab iis solis legantur qui principia prius evolverint" (p. 401). Moreover, as mentioned by Edleston, there is actually the manuscript of the work, partly in Roger Cotes's handwriting, in the library of Trinity College, Cambridge.

In 1777 appeared: *Mathematical Principles of Natural Philosophy by Sir Isaac Newton, Knight. Translated into English and illustrated with a Commentary by Robert Thorp, M.A. Volume the First.* This translation covers the whole of Book I. of the *Principia.* No further volume appeared, and in a second edition, dated 1802, a change in the title, except for which the edition is a mere reprint, shows that Thorp had abandoned his ambitious plan.

A translation of the first book was edited by Charles Thorp, with a full commentary (1777 and 1802), and of the first three sections by John Carr (1821 and 1825), J. M. F. Wright (1830), J. H. Evans (five editions between 1834 and 1871, the last being by P. T. Main), G. L. Cook (1850), and Percival Frost (1854), of which a fifth edition appeared in 1900.

The only contemporary foreign translation was a French one, which was published in two volumes in 1759 as *Principes Mathématiques de la Philosophie Naturelle, par feue Madame la Marquise du Chastellet.* It will be seen that Newton's name is not even mentioned on the title.

The translation is a very free one, for as the *Avertissement* explains, "L'illustre interprète, plus jalouse de saisir l'esprit de l'auteur que ses paroles, n'a pas craint en quelques endroits d'ajouter ou de transposer quelques idées pour donner au sens plus de clarté. En conséquence on trouvera souvent Newton plus intelligible dans cette traduction que dans l'original, et même que dans la traduction angloise." The second volume consists of a valuable commentary, mainly the work of Clairaut, who also added a synopsis of his treatise on the figure of the earth. There is also (in vol. i.) an interesting historical preface by Voltaire, the translator's friend, and (in vol. ii.) Daniel Bernoulli's prize essay on the tides, "augmenté de diverses notes et eclaircissemens que l'auteur a communiqués."

A German translation, by Jacob Philipp Wolfers, was published in 1872.

Like all great discoveries, those contained in the *Principia* were but slow in making their way, and in displacing the Cartesian system of vortices, then taught in the English universities. If Voltaire's statement be believed, Newton, at his death, had no more than twenty followers in England, while the teaching of the Cartesian philosophy, according to Lord Playfair, still prevailed in the English universities more than thirty years after the first appearance of the *Principia.*

So well established was it at Cambridge at the beginning of the Eighteenth Century, that the small editions of the *Principia* were found quite sufficient, while Samuel Clarke's translation of Rohault's text-book of Cartesian physics went through several editions. Yet it was this very book which finally helped to dislodge it, through the stratagem of introducing judicious notes, contrasting the Cartesian with the Newtonian philosophy, and thus making the fallacies of the former apparent.

Several books were published on the *Principia,* the most important being S. P. Rigaud's *Historical Essay on the First Publication of Sir Isaac Newton's Principia* (1838), containing unpublished correspondence, and a reprint of the treatise *De Motu*; W. W. R. Ball's *Essay on Newton's Principia* (1893), which also gives a reprint of *De Motu,* besides a full analysis and the correspondence between Newton, Hooke, and Halley; P. G. Tait's *Newton's Laws of Motion* (1899); and Dr. Glaisher's valuable *Bi-centenary of Newton's Principia* (1888). There is also an interesting historical account of the first two editions in J. Edleston's *Correspondence of Sir Isaac Newton and Professor Cotes* (1850).

In 1867 the scientific world was startled by the news that Michel Chasles, the well-known mathematician, had in his possession autograph letters which conclusively proved that the discovery of the law of universal gravitation was made by Pascal, and communicated to Newton, who was charged with having fraudulently published it as his own. Sir David Brewster, after seeing the pretended autograph letters, immediately pronounced them to be forgeries, and stated it publicly in an address at the meeting of the British Association in 1867. A full exposure, with facsimiles of the forged letters, was published in 1868 by Prosper Faugère in *Défense de B. Pascal, et accessoirement de Newton, Galilée, Montesquieu, etc., contre les faux Documents présentés par M. Chasles à l'Académie des Sciences.*

THE CALCULUS.

The invention of the calculus, like the discovery of the law of universal gravitation, was made in 1665, when Newton was staying at Woolsthorpe, whither he had retired on account of the Plague. This fact he mentioned himself in the mathematical treatises appended to his *Opticks* of 1704. Moreover, a dated manuscript in Newton's handwriting shows that he already used fluxions—to find the tangent and the radius of curvature at any point of the curve—as early as November, 1665. The first account of the invention was contained in a manuscript afterwards published as *De Analysi per Æquationes*, which was communicated in 1669 to Isaac Barrow, and by him to John Collins, who circulated it among Newton's friends. No printed account of the calculus appeared until 1687, when the fundamental principles, with a method to find the moments of the products of any power whatsoever, were published in Lemma II in the second book of the *Principia*. A fuller account, and the first giving the fluxional notation, was contained in two letters from Newton to John Wallis, which were printed in 1693 in the second volume of the latter's *Opera Omnia.*

The first independent publication appeared as an appendix to Newton's *Opticks* of 1704, as *Two Treatises of Species and Magnitude of Curvilinear Figures*, and here he publicly reveals his unwillingness to be drawn into controversies, when he states in the " Advertisement " that he had delayed the printing " to avoid being engaged in disputes about these matters."

The first of these treatises was the *Enumeratio Linearum Tertii Ordinis,* which gives a classification of seventy-two curves of the third order, with their properties. W. W. R. Ball says that " Newton shows that many of the most important properties of conics have their analogues in the theory of cubics, and discusses the theory of asymptotes and curvilinear diameters." A reprint of the work, with James Stirling's *Illustratio,* was published at Paris in 1797, and an English translation by C. R. M. Talbot, M.P., F.R.S., in 1860.

The second treatise was the *Tractatus de Quadratura Curvarum,* which describes the method of fluxions, and contains the first printed account of the binomial theorem. It gives Newton's method of effecting the quadrature and rectification of curves by means of infinite series, and contains the earliest use in print of literal indices (*Ball*). It is also noteworthy for the fact that Newton, who had hitherto based his calculus on infinitesimals not unlike those of Leibniz, here first adopts fluxions. The second tract was reprinted at Upsala in 1762, and a translation of it, together with that of *Analysis per Æquationes,* by John Stewart of Aberdeen, was published in 1745.

In 1711 was published, under the editorship of William Jones, the ***Analysis per Quantitatum Series, Fluxiones ac Differentias, cum Enumeratione Linearum Tertii Ordinis.*** Besides reprints of " Enumeratio " and " Tractatus de Quadratura," above mentioned, this contains, for the first time, the important ***Methodus Differentialis,*** which gives Newton's method of interpolation, the *De Analysi per Æquationes,* which is substantially the same as " Tractatus de Quadratura Curvarum," first published in *Opticks* (1704), and the ***Fragmenta Epistolarum ad D. Oldenburgum.*** The editor's preface gives early applications of the fluxional calculus. The work was reprinted in the Amsterdam re-issue of the *Principia* in 1723.

The last work published on the calculus, and the one giving the fullest exposition of it, was the unfinished *Methodus Fluxionum.* It had a curious history. It was originally intended that it should be published in 1671 as an introduction to a new edition of Kinckhuysen's *Algebra,* which Newton then meant to bring out. The scheme, however, fell through, and it was then proposed that it should accompany Newton's work on the theory of colours which was to have been published in the same year ; but as Newton found himself overwhelmed with objections to his paper on the theory of colours, he dropped the project. Again, in 1680, Collins intended to publish it, but for unknown reasons never carried out the plan, and the work first saw the light in an English translation in 1736 as *The Method of Fluxions and Infinite Series : with its Application to the Geometry of Curve-Lines, translated from the Latin Original not yet made publick. With a perpetual Comment, containing the Annotations, Illustrations and Supplements, by John Colson, F.R.S.* Another translation, but without Colson's commentary, appeared in 1737 as *A Treatise on the Method of Fluxions and Infinite Series,* etc.

This work first contains Newton's parallelogram, his method of approximating to the roots of numerical equations, and first explains the expansion into series of fractional and irrational quantities.

Colson's edition formed the basis of Buffon's translation, which came out in 1740, but without Buffon's name, as *La Méthode des Fluxions, et des Suites Infinies.* Buffon's preface is of great interest, as it gives an account of the early history of the invention of the calculus, and for the statement contained in it that Newton was its only inventor (" on demeura convaincu que Newton seul est l'auteur de ces merveilleux calculs ").

The Latin original did not appear until 1779, when it was published in the first volume of the *Opera Omnia* as *Geometria Analytica.*

The best account of Newton's writings on pure mathematics is contained in Cantor's *Vorlesungen über Geschichte der Mathematik,* vol. iii. There is some confusion between the " Methodus Fluxionum " and the " Analysis per Quantitatum Series " in Ball's *History of Mathematics.*

One other important mathematical work, not connected with the calculus, was published in 1707 as *Arithmetica Universalis : sive de Compositione et Resolutione Arithmetica Liber, cui accessit Halleiana Æquationum Radices arithmetice inveniendi Methodus.* It was published by William Whiston, and gives the substance of the algebraic lectures Newton read from his Lucasian chair for the first nine years. It contains " Newton's Rule," and a number of other important theorems in the theory of equations. A second edition, with additions, appeared in 1722, and a Continental one in 1732, edited by W. J. 's Gravesande, with excerpts from Halley, Colson, de Moivre, Maclaurin, Kästner, and Boscovich on the theory of equations ; another by J. A. Lecchi in 1752, a third with a 288-page commentary by Joh. Castillioneus [Giovanni Salsemini] in 1761, and an abridgment by G. A. Décoré in the same year.

An English translation by Joseph Raphson, revised by Samuel Cunn, came out in 1720, 1728, and (with additions by J. Maguire and T. Wilder) in 1769. A French translation by Noël Beaudeux was published in 1802.

THE *COMMERCIUM EPISTOLICUM.*

No dispute on an invention or discovery in pure science, not touching on religious dogma, was ever carried on with such bitterness, none enlisted such a large number of eminent scientists, or left behind such a long estrangement between the two camps into which the combatants separated, than that on the claim to priority in the invention of the calculus.

That Newton invented and used it as early as 1665 there is now no doubt, and had it not been for an excessive sensitiveness inherent in his character, which made him shrink from every controversy, he would not have withheld its publication, and no controversy could ever have arisen.

The facts that led to the publication of the *Commercium Epistolicum* are shortly these. When Leibniz first came to London in 1673, he became acquainted with the foremost English mathematicians, and more especially with his countryman Henry Oldenburg, the Secretary of the Royal Society, with whom he kept up a regular correspondence afterwards.

On June 13th, 1676, Newton wrote a long letter to Leibniz, in which he describes his method of series. This was followed by a second letter, dated October 24th, 1676—but not sent to Leibniz until March, 1677—giving a full account of his method as discovered in 1665. In 1676, Leibniz was also shown by John Collins the manuscript of Newton's *De Analysi per Æquationes.* Leibniz admitted having seen part of the manuscript, but denied that the portion shown to him contained an explanation of the method of fluxions.

Leibniz claimed to have invented the differential and integral calculus in 1674. It was not developed, however, into a consistent system until 1677, nor was it published (in the *Acta Eruditorum*) until 1684, or twenty years before Newton published his method. On this fact of the priority of publication, Leibniz mainly based his claim to the independent invention of the calculus. In this paper, which may be said to have opened the long controversy, Leibniz also asserts that certain mathematical problems can be solved only by means of his own or a "similar" calculus, without mentioning Newton's name. This very probably induced Newton to insert the Lemma in the *Principia* of 1687, in which he gives an account of his own calculus and refers to Leibniz's invention. Leibniz alterwards claimed that Newton by doing so had acknowledged his invention to be independent of Newton's.

In January, 1705, appeared an anonymous review in the *Acta Eruditorum*—actually written by Leibniz—of Newton's *Tractatus de Quadratura Curvarum,* published in 1704. This contained the offensive assertion that Newton had employed fluxions instead of the differential calculus in the same way as Fabri in his *Synopsis Geometriæ* had substituted a method of progressive motion for Cavalieri's method of indivisibles, thus implying that Newton had borrowed his method from Leibniz's. This charge of plagiarism was answered by John Keill, in the *Philosophical Transactions* for 1708, in which Newton is declared "beyond all doubt" to be the first inventor of fluxions.

The quarrel now dragged on with increasing bitterness, especially on Leibniz's part, and culminated in a report on the question of priority by a committee appointed by the Royal Society. The committee formed for the purpose were "to examine the registers of the Society, and to lay before it such documents as they might discover, with their own opinions on the subject." This formed the substance of the *Commercium Epistolicum D. Johannis Collins, et aliorum de Analysi promota: Jussu Societatis Regiæ in lucem editum,* which was published in 1712. In addition to the correspondence on the subject in question, it contains a Recensio of forty-one pages, giving an abstract of the publication, which was written by Newton himself, and a reprint of the manuscript *De Analysi per Æquationes,* which was shown by Collins to Leibniz. Only a very small number of copies were printed, chiefly for presentation, and the work became shortly so rare that Raphson declared in 1715 that the work "was not to be met with among booksellers."

A second edition was published in 1722, *cum ejusdem* [Newtoni] *recensione præmissa, et judicio primarii, ut ferebatur, mathematici subjuncto.* It contains besides other additions a long new address to the reader by Newton. That the recensio and address were by Newton was first pointed out by Prof. de Morgan. This edition was reprinted in 1725 in two issues, one of which contains the significant cancellation of Leibniz's name, which was repeated in the Lemma on fluxions in the third edition of the *Principia,* published in the following year.

A reprint of the first edition was issued in 1856 by J. B. Biot and F. Lefort, "avec l'indication des variantes de 1722," a "collection de pièces justificatives et de documents," which in the editors' opinion justified Leibniz's claim, as well as excerpts from the works of Cavalieri, Descartes, Fermat, Hudde, Ricci, Barrow, and Sluze that ultimately led to the invention of the calculus.

Many works were published on the controversy, the most important contemporary ones being *Virorum celeberrimorum G. Gul. Leibnitii et Joh. Bernoullii Commercium Philosophicum et Mathematicum,* 1694-1716 (1745), the *Recueil de diverses Pièces sur la Philosophie, la Religion Naturelle, l'Histoire, les Mathématiques, etc., par Mrs. Leibniz, Clarke, Newton, etc.* (1720), which was edited by Pierre des Maizeaux, and contained the correspondence on the dispute between Leibniz and the Abbé Conti, and Joseph Raphson's *The History of Fluxions, showing in a compendious Manner the first Rise of, and various Improvements made in that incomparable Method* (1715), containing important correspondence on the dispute. The last was also published in Latin in the same year.

The case for Newton was given in H. Sloman's *Leibnitzens Anspruch auf die Erfindung der Differenzialrechnung* (1857), of which an English translation, with additions, was published in 1860; and for Leibniz in G. J. Gerhardt's *Die Entdeckung der Differenzialrechnung durch Leibniz* (1848), and in various papers by Prof. de Morgan, while the case was summed up in P. Mansion's *Esquisse de l'Histoire du Calcul infinitésimal* (1887).

MINOR WORKS.

In 1728 was published the *Chronology of Ancient Kingdoms amended. T which is prefix'd, a Short Chronicle from the First Memory of Things in Europe, to the Conquest of Persia by Alexander the Great.* The original draft of the manuscript, consisting only of twenty-four pages, was lent in 1724, under a promise of secrecy, to the Abbé Conti, Leibniz's friend. The promise, however, was broken, and the manuscript given to Nicolas Fréret, who translated it and added notes of his own, which were supposed to refute Newton's theories. This was printed in 1725 as *Abrégé de Chronologie de M. le Chevalier Newton, fait par lui-même, et traduit sur le Manuscript Anglois.* A full answer by Newton to Fréret's attack appeared in the *Philosophical Transactions* for 1725 as *Remarks on the Observations made on a Chronological Index of Sir Isaac Newton, translated into French by the Observer, and published at Paris.* It contains interesting references to the connexion between Conti and Leibniz.

As a further answer to Fréret, and to Etienne Souciet, S.J., who had violently attacked the work in 1726, Newton enlarged it, and prepared it for the press. It was published by Mr. Conduitt in 1728. The Chronology is divided into six chapters, and is remarkable for first basing the principles of chronology on astronomical observations, rather than on the evidence of monuments and tradition. In writing the long dedication to the Queen, Conduitt had the help of Pope. A French translation was published in 1728, and an Italian in 1757.

Newton's chief theological writings are: (1) *Observations upon the Prophecies of Daniel and the Apocalypse of St. John,* published in 1733, and (2) *Historical Account of Two Notable Corruptions of Scripture,* which was first printed in vol. v of the *Opera Omnia,* and reprinted, in the Socinian interest, in 1830. They are of interest in connexion with the charge of anti-Trinitarianism which was made against Newton. A full analysis of the two works will be found in Brewster's *Life* (1860), vol. ii, pp. 263-76.

Two works were edited by Newton. One was the work of Isaac Barrow, his teacher: *Lectiones XVIII, in quibus Opticorum Phenomenωn genuinæ Rationes investigantur, ac exponuntur.* It was first published in 1669, and contains many additions by Newton, who also revised and corrected the work. The second was Bernhard Varenius's *Geographia Generalis, in qua Affectiones* *generales Telluris explicantur, summâ Curâ quam plurimis in Locis emendata et XXXIII Schematibus novis, ære incisis, unà cum Tabb. aliquot quæ desiderabantur aucta et illustrata ab Isaaco Newton*, published at Cambridge in 1672. The translator's preface to the third English edition, published in 1736, mentions that "The reason why this great man took so much care in correcting and publishing our author, was because he thought him necessary to be read by the audience while he was delivering lectures upon the same subject from the Lucasian chair."

Newton did not escape the penalty of greatness, when, after his death, the following work was impudently foisted on him in 1729: *Sir Isaac Newton's Tables for Renewing and Purchasing the Leases of Cathedrals, Churches and Colleges, according to the Several Rates of Interest: with their Construction and Use explained. Also Tables for Renewing and Purchasing the Leases of Land or Houses. Very necessary and useful for all Purchasers, but especially those who are in any way concerned in Church or College Leases.* The supposititious authorship must have proved very attractive, for the work went through many editions.

COLLECTED WORKS.

A collected edition of Newton's works was published at Lausanne and Geneva in 1744, in three volumes, as *Opuscula Mathematica, Philosophica et Philologica, collegit partimque Latinè vertit Joh. Castillioneus.* It gives all the works, with the exception of the *Arithmetica Universalis*, the *Principia*, the *Opticks*, and the *Methodus Fluxionum*, and has in a Latin translation all the papers published in the *Philosophical Transactions*. Of special interest is the paper *De Natura Acidorum*, which is contained in it. Together with his chemical queries in the *Opticks*, it is the only chemical work Newton ever published. Gray's *Bibliographia* only mentions it as forming part of the *Opuscula*, and Brewster, who merely gives the title, seemingly did not know when and where it was published. It actually first came out, in Latin and English, in the second volume of John Harris's *Technical Dictionary*, published in 1710.

By adding to the above work the editor's edition of the *Arithmetica Universalis*, and the "Jesuits'" edition of the *Principia*, one can get a fairly complete and uniform edition of the works.

The fullest edition hitherto published—although by no means a complete one, such as the title implies—came out in five volumes in 1779-85, as *Opera quæ extant Omnia, commentariis illustrabat Samuel Horsley, R.S.S.* They contain all the printed work of Newton issued in book form, the paper *De Natura Acidorum*, a number of his Letters, some in extracts, and the hitherto unpublished *Short Chronicle from a MS. of Sir Isaac Newton in the Possession of the Rev. Dr. Ekins, Dean of Carlisle.* It also gives, for the first time, the original text of the unfinished *Methodus Fluxionum*, which was edited from three different manuscripts. The editor divided the work into twelve chapters, and gave it a new title: *Artis Analyticæ Specimina, vel Geometria Analytica.* The work also contains three original mathematical papers by the editor.

It will be seen that the *Opera Omnia* do not contain the papers published in the *Philosophical Transactions*, nor do they give the complete correspondence then available, or the tract *De Motu*, or other important papers from the Portsmouth Collection, or Newton's work at the Mint.

It is to be hoped that the occasion of the Bicentenary of Newton's death will give the stimulus for a work long overdue, that of a truly complete, a "National" Edition of Newton's works, one worthy of so great a genius.

THE PORTSMOUTH COLLECTION.

No Newton Bibliography would be complete that did not mention the collection of unpublished manuscripts and annotated works which are generally known as the Portsmouth Collection.

They came after Newton's death into the possession of Mr. Conduitt, who had married Catharine Barton, Newton's step-niece, and eventually into that of Lord Portsmouth at Hurstbourne. They were inspected by Samuel Horsley when preparing his edition of Newton's works, but ignored. Later again the collection was seen by Sir David Brewster, who made some use of it in his *Life of Newton.*

In 1872 the collection was presented by the Earl of Portsmouth to the University of Cambridge, with a request to return all papers on theology, chronology, history, and alchemy, as well as those relating to private, personal, and family matters.

The University appointed a syndicate to examine, classify, and divide the collection. This was done, and an excellent report prepared, which was printed as *A Catalogue of the Portsmouth Collection of Books and Papers written by or belonging to Sir Isaac Newton, the Scientific Portion of which has been presented by the Earl of Portsmouth to the University of Cambridge* (Cambridge, 1888).

Among the mathematical papers are three manuscripts (two incomplete) of the treatise *De Motu,* which formed the basis of the first book of the *Principia.* Of importance are the papers on the Lunar Theory. They contain some which were probably intended to be inserted in the second edition of the *Principia,* while others, of which only the results are given in the book, are here fully worked out.

Other important papers are those on the theory of atmospheric refraction, of which the table only was published by Halley in 1721 in the *Philosophical Transactions.* Of special interest are those on the Newton-Leibniz Controversy, and Newton's correspondence, mostly unpublished.

The chemical papers and abstracts included might still be worth special study, for it is well known that Newton took a great interest in the science, and experimented from early college days, but his only chemical works published were the chemical queries in the *Opticks,* and the paper *De Natura Acidorum.* There are also many abstracts from writers on alchemy, in which Newton took a special interest in his later years.

A full account of the collection will be found in the above-mentioned *Catalogue of the Portsmouth Collection.*

BIOGRAPHIES AND CORRESPONDENCE.

The earliest Life is Fontenelle's *Éloge de M. le Chevalier Newton,* published in 1728. It is mainly based on materials supplied by Mr. Conduitt. Three English translations of this were published in the same year. An anonymous *Life of Newton* was issued in the *Universal Magazine* in 1748. Pemberton's and Maclaurin's accounts are mentioned in the chapter "Commentators," and the Lives by W. Davis and N. W. Chittenden are referred to among the translations of the *Principia.* Benjamin Martin's *Biographia Philosophica* (1764) contains a "Life of Newton," with a portrait. An Italian appreciation appeared in 1778 in Paolo Frisi's *Elogio del Cav. I. Newton.* Edmund Turnor's *Collections for the History and Soke of Grantham, containing authentic Memoirs of Sir Isaac Newton, now first published from the original MSS. in the Possession of the Earl of Portsmouth* (1806) contains valuable original information on Newton's early life and family, and first gives a sketch of the life drawn up by John Conduitt, the original of which is in the Portsmouth Collection.

Biot's Life, published in the *Biographie Universelle,* is chiefly remarkable for attempting to prove that Newton was incapable of doing any important scientific work after his illness in 1692-3, a view fully rebutted by Brewster. Lord Brougham's *Life of Sir Isaac Newton* (1829) is substantially a translation of Biot's, and contains a bibliography. Important and original sketches on Newton were published by Prof. De Morgan between 1846 and 1858, and edited by P. E. B. Jourdain under the collective title *Essays on the Life and Work of Newton* (Chicago, 1914).

The first satisfactory Life was Sir David Brewster's *The Life of Sir Isaac Newton*, published in 1831, and consisting of 366 pages. This was afterwards much enlarged, and issued in 1855 in two volumes, as *Memoirs of the Life, Writings and Discoveries of Sir Isaac Newton.* It is remarkably complete, and the standard life, and probably will remain so for many years. Its only defect is a rather scanty index, which makes the use of the work difficult for reference. A second edition, slightly enlarged but in a smaller print and size, came out in 1860. There is also a valuable article on Newton, by Sir Richard T. Glazebrook, in the *Dictionary of National Biography.* The best account of Newton's scientific work is to be found in *Isaak Newton und seine physikalischen Principien : ein Hauptstück aus der Entwickelungsgeschichte der modernen Physik* (1895), by Ferdinand Rosenberger, the well-known historian of physics.

Newton's correspondence has never been published in a connected work, and can only be found scattered through many works, the most important of which are, besides those mentioned under the chapter " Commercium Epistolicum," T. Birch's *History of the Royal Society of London* (4 vols. 1756), S. P. Rigaud's *Correspondence of Scientific Men of the Seventeenth Century, including Letters of Barrow, Flamsteed, Wallis, and Newton, published from the Originals in the Collection of the Earl of Macclesfield* (2 vols. 1841), a valuable collection, originally in the possession of William Jones, the *Correspondence of Richard Bentley* (2 vols. 1842), Leibniz's *Mathematische Schriften, herausgegeben von C. I. Gerhardt* (2 vols. 1849), and J. Edleston's valuable *Correspondence of Sir Isaac Newton and Professor Cotes, including Letters of other eminent Men, now first published, with an Appendix of unpublished Letters and Papers by Newton, with Notes, a Synoptical View of his Life,* etc., etc. (1850), which is of special importance in connexion with the first two editions of the *Principia.*

A thinly disguised attempt to detract from Newton's fame was made by the publication of the correspondence turning on Newton's quarrel with Flamsteed, by Francis Baily, in *An Account of John Flamsteed, compiled from his own MSS., and other authentic Documents, never before published, to which is added his British Catalogue of Stars, corrected and enlarged, by Francis Baily* (1835-7), which suppressed not only letters but passages that were in Newton's favour. It must now appear strange that the work should have been printed " by Command of the Lords Commissioners of the Admiralty," at the public expense.

Of bibliographies there is the painstaking *Bibliography of the Works of Sir Isaac Newton, together with a list of Books illustrating his work, with Notes by George J. Gray, second Edition, revised and enlarged* (1907).

COMMENTATORS.

The *Principia* is by no means easy reading. " The demonstrations throughout the work," says W. W. R. Ball, " are geometrical, but to readers of ordinary ability are rendered unnecessarily difficult by the absence of illustrations, and by the fact that no clue is given to the method by which Newton arrived at his results." Commentaries, therefore, became necessary and were abundantly supplied in the course of the Eighteenth Century.

The earliest introduction was published in 1700, by John Keill, in his *Introductio ad veram Physicam,* of which an English edition was published in 1720. David Gregory's *Astronomiæ Physicæ et Geometricæ Elementa,* first published at Oxford in 1702, was the first text-book on gravitational principles. An English edition, including Halley's " Synopsis of the Astronomy of Comets," was published in 1715. A short exposition of the fundamental theorems of the *Principia* was contained in Humphrey Ditton's *General Treatise on the Laws of Nature and Motion . . . being part of the great Mr. Newton's Principles*

(1705). The first semi-popular exposition is given in *Prælectiones physico-mathematicæ Cantab. in Pub. Schol. habitæ. Quibus Philosophia Newtoni Mathematica explicatius traditur et facilius demonstratur*, by William Whiston, Newton's successor in the Lucasian chair. It appeared in 1710. An English translation, corrected and improved by the Author, came out in 1716. In 1728 appeared *A View of Sir Isaac Newton's Philosophy*, by Henry Pemberton, the editor of the third edition of the *Principia.* It gives in the preface the author's recollections of Newton at an advanced age. The first poetical effusion is due to J. T. Desaguliers, in *The Newtonian System of the World, an allegorical Poem* (1728). Two commentaries on the *Principia* were published in 1730: *Demonstration of some of the principal Sections of Sir Isaac Newton's Principles of Natural Philosophy*, by John Clarke, and Georg Peter Domcke's *Philosophiæ Mathematicæ Newtonianæ illustratæ Libri II.*

Commentaries thenceforward became numerous, and only Colin Maclaurin's *Account of Sir Isaac Newton's Philosophical Discoveries* (1748), James Ferguson's *Astronomy explained upon Newton's Principles, and made easy* (1756), and Emerson's *Short Comment on Sir Isaac Newton's Principia* (1770) need be specially mentioned.

The first important Continental text-book on Newtonian principles was 's Gravesande's *Philosophiæ Newtonianæ Institutiones, in Usus academicos* (1723) and his *Physices Elementa Mathematica, Experimentis confirmata, sive Introductio ad Philosophiam Newtonianam* (1720). In the same year appeared an English translation of this by J. T. Desaguliers.

In 1738 came out a work which did much to displace the Cartesian philosophy then still generally taught in France. It was Voltaire's *Élémments de la Philosophie de Neuton* [*sic*] *mis à la Portée de tout le Monde.* The book was written in 1736, but only the first seventeen chapters are Voltaire's, the completion being by a Dutch mathematician. So intensive were the publisher's efforts in circulating the work, that it was jocularly remarked, there must be a misprint on the title, which should read "mis à la porte de tout le monde." An English translation by John Hanna, "with Explications of some Words in alphabetical Order," was published in the same year. The work was afterwards wholly rewritten, and therein first appeared the story of the falling apple, which Voltaire, when staying in London, had from Mrs. Conduitt. Another work which helped to displace the teaching of Cartesian philosophy was Pierre Sigorgne's *Institutions Newtoniennes*, first published in 1747.

In Germany appeared a *Geschichte der Entdeckung der allgemeinen Gravitation durch Newton*, by J. J. von Littrow, in 1735.

The first truly popular account, and an excellent one, of Newton's optical discoveries, with a chapter on attraction, was Francesco, Conte di Algarotti's *Il Newtonianismo per le Dame, ovvero Dialoghi sopra la Luce e i Colori*, published anonymously in 1737. It was translated into English in 1739 as *Sir Isaac Newton's Philosophy Explain'd for the Use of Ladies, in six Dialogues on Light and Colours. From the Italian of Sig. Algarotti.* Popularisations were also published by Benjamin Martin, polygraph and mathematical instrument maker, from 1747 onwards.

There only remained to be published a Newton for the Young, and this was supplied by John Newbery, the publisher of Goldsmith and Dr. Johnson, when he issued *The Newtonian System of Philosophy, adapted to the Capacities of Young Gentlemen and Ladies, and familiarized and made entertaining by Objects with which they are intimately acquainted : the Substance of Six Lectures read to the Lilliputian Society by Tom Telescope, A.M., and collected and methodized for the Benefit of the Youth of these Kingdoms, by their Old Friend, Mr. Newbery, in St. Paul's Churchyard.* The work was very successful, and went through many editions (the fourth being published in 1770), but is now very rare, it must be assumed owing to the destructiveness of the young.

PARADOXERS.

While Newton's discoveries became popularised, and gradually changed the trend of scientific thought, the paradoxer could hardly fail to contribute his mite of wisdom. Nor did he. Indeed, it may be doubted whether any other system called forth such a steady flow of paradoxes as did Newton's law of gravitation. The inverse square of the distance appears at an early date to have become an obsession, and under its influence was written John Craig's *Theologiæ Christianæ Principia Mathematica* (1699), which "proves" that the evidence in favour of the truth of the Gospel narrative will become zero in A.D. 3150. In 1719 appeared George Gordon's *Remarks upon the Newtonian Philosophy, wherein the Fallacies of the pretended Mathematical Demonstrations are clearly laid open, and the Philosophy itself fully proved to be false and absurd, by Mathematical and Physical Demonstration.* The work was successful, for a second edition was published with a long preface, "shewing how a Set of Men, whose Characters and Interests depend upon the Reputation of that Philosophy, endeavour to support it in the Eyes of the World against these Remarks, by indirect Management, for Want of Reason and Argument."

A powerful school of paradoxers arose in the "Hutchinsonians," which included Duncan Forbes, John Parkhurst, George Horne, Bishop of Norwich, and William Jones of Nayland. It was started in 1724 by John Hutchinson's *Moses's Principia. Of the Invisible Parts of Matter; of Motion; of Visible Forms; and of their Dissolution and Reformation*, which denied the theory of gravitation as involving the existence of a vacuum, and denounced it as being of dangerous consequence. A defence of the work was published in 1753 by George Horne in *A Fair, Candid, and Impartial State of the Case between Sir Isaac Newton and Mr. Hutchinson, in which it is shown how far a System o Physics is capable of Mathematical Demonstration: how far Sir Isaac's, as such a System, has that Demonstration, and consequently, what regard Mr. Hutchinson's Claim may deserve to have paid to it.*

A sensational discovery was made by Charles Palmer, who published it in 1798 as *A Treatise on the Sublime Science of Heliography, satisfactorily demonstrating our Great Orb, the Sun, to be absolutely no other than a Body of Ice! Overturning all the received Systems of the Universe hitherto extant, proving the celebrated and indefatigable Sir Isaac Newton, in his Theory of the Solar System, to be as far distant from Truth, as any of the Heathen Authors of Greece and Rome.*

Early in the nineteenth century arose the astronomical school of Richard Brothers, "the prophet," as he was called, who declared that he was to be revealed as prince of the Hebrews and ruler of the world, and that King George must deliver up his crown to him. His contribution to astronomy came out in 1801 as *Description of Jerusalem, its Houses and Streets, Squares . . . with the Garden of Eden in the Centre, as laid down in the last Chapters of Ezekiel: also the first Chapter of Genesis verified . . . , and the Solar System, with all its Plurality of Inhabited Worlds, and Millions of Suns, as positively proved to be delusive and false.* Bartholomew Prescot, one of the most enthusiastic followers of the "prophet," published in 1803, *A Defence of the Divine System of the World, which represents the Earth as being at Rest, and the Heavenly Bodies in Motion about it; with a Demonstration of the Fallacy of the Solar System, invented by Pythagoras, revised by Copernicus, and established by Sir Isaac Newton.*

Another follower was John Finlayson, who published, at the "Age of the World, 5502; or Christian Aera, 1832," *The Universe as it is, in Support of the Scriptures, or the complete Refutation and Exposure of the false and fabricated Solar System, with the true Account of the Moon's and Planets' variable Motions, and how to find the Longitude, with . . . the Discovery of the Ten Tribes of Israel.* The earth, the author says, is a perfect sphere, "not shaped like a garden turnip, as the Newtonians make it," the sun is "very different from anything we can make here below," and the stars are "oval-shaped immense masses of frozen water, with their largest ends foremost." About the same time he published *God's Creation of the Universe as it is, in Support of the Scriptures.*

Another author who was sorely troubled about the erroneous views on gravitation was Captain Forman, R.N., but when he found that no attention was paid to his epistolary advice by the eminent scientists to whom he addressed it, and when the Astronomical Society actually advised him to study dynamics, he retaliated, exasperated beyond endurance, by publishing his *Letter to the Royal Astronomical Society in Refutation of Mistaken Notions held in common by the Society, and by all the Newtonian philosophers* (1833), in which, after relieving his mind by calling the Council of the Society "craven dunghill cocks," he showed them the error of their ways.

Sir Richard Phillips, the publisher, was a paradoxer who combined business with inexact scientific information. In 1818 he brought out his *Essays on the Proximate Mechanical Causes of the General Phenomena of the Universe*, which was re-issued in 1821, in an enlarged form, as *Twelve Essays on the Proximate Causes of the Material Phenomena of the Universe, with illustrative Notes.* Professor de Morgan says that "Phillips was not only an anti-Newtonian, but carried to a fearful excess the notion that statesmen and Newtonians were in league to deceive the world."

One of the last of the prominent anti-Newtonian paradoxers was Richard James Morrison, better known to astrologers under his pseudonym of Zadkiel, who published in 1857 *The Solar System as it is, and not as it is represented: wherein is shewn for the first Time, the true proper Motion of the Sun through Space, at the Rate of* 100,000 *Miles per Hour; also that the Earth and Planets, and their Satellites move with the Sun, in Cycloidal Curves, and that the Doctrine of Elliptical Orbits is false; being an Optical Illusion that has arisen from Ignorance of the Sun's Motion through Space,* in which "the complex fallacies of the Newtonian Theories of Gravitation and Attraction, and their sister folly, the Keplerian Doctrine of Ellipses, are now shown to be utterly unfounded."

A list of prominent Newtonian paradoxes should also include the standard work on the flat-earth paradox. It was due to Parallax, a pseudonym which has been variously identified with Samuel Rowbotham, M.D. and an S. Goulden. It was published in 1864 as *Zetetic Astronomy. Earth not a Globe! An Experimental Inquiry into the True Figure of the Earth, proving it a Plane, without Axial or Orbital Motion; and the only Material World in the Universe.*

Another anti-Newtonian flat-earth paradox was Louis Sébastien Mercier's *De l'Impossibilité du Système Astronomique de Copernic et de Newton* (1806). It should gain additional interest from the fact that its author was a Member of the French Institute.

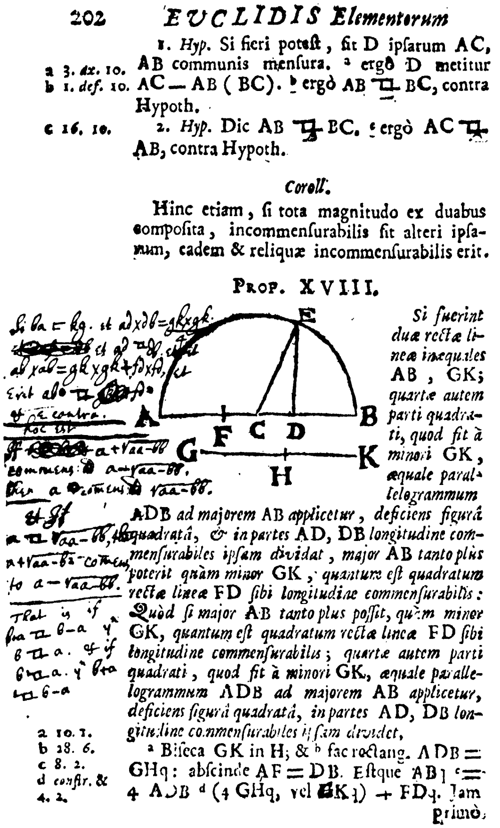
If England was the home of the anti-Newtonian astronomical paradox, so was the Continent of Europe that of the optical variety. Its chief representative is Celestino Cominale's *Anti-Newtonianismus, Partes I. et II., in quibus Newtoni de Coloribus Systema ex propriis Principiis geometrice evertitur: et Nova de Coloribus Theoria luculentissimis Experimentis demonstrantur . . .* (Neapoli, 1754-6).

Verging on a paradox is *L'Optique des Couleurs, fondée sur les simples Observations, et tournée surtout à la Pratique de la Teinture, et des autres Arts coloristes* (1740), by Louis Bertrand Castel, S.J., in which black is said to be a combination of "dark" colours, and white of "light" colours. This volume may have been the germ of Goethe's ill-conceived *Farbenlehre* (1810), wherein colours are declared to be formed of white and black in varying proportions. The work is still suggestive to artists, but is now chiefly of a pathetic interest, for it embittered the last years of the poet when he found crumbling to pieces what he had considered the crown of his life, and valued more than his literary work.

The progress of science has not yet put a stop to the paradox, and fresh examples appear almost every year. Those who take an interest in this curious by-path of science will find rich fare in De Morgan's *Budget of Paradoxes.*

NEWTON'S LIBRARY.

Habent sua fata libelli. This is certainly true of the books that once constituted Newton's library. That he must have possessed a fairly large library can scarcely be doubted, yet it is a curious fact that none of his biographers refers to it. Brewster mentions some of the books which Newton bought, and others which were presented to him, but without giving a clue as to their ultimate fate. The Portsmouth Collection contains a few books, but none of those mentioned by Brewster. The question would now arise: What became of those books, were they dispersed without their ownership being made known, and were they thus lost to posterity?



202 *EUCLIDIS Elementorum*

1. *Hyp.* Si fieri poteſt, ſit D ipſarum AC, AB communis menſura. [a] ergò D metitur AC — AB (BC). [b] ergò AB ⊓ BC, contra Hypoth.

2. *Hyp.* Dic AB ⊓ BC. [c] ergò AC ⊓ AB, contra Hypoth.

a 3. *ax.* 10.
b 1. *def.* 10.
c 16. 10.

Coroll.

Hinc etiam, ſi tota magnitudo ex duabus compoſita, incommenſurabilis ſit alteri ipſarum, eadem & reliquæ incommenſurabilis erit.

PROP. XVIII.

Si ba ⊏ kg. et ad×db=gk×gk.
[illegible] et ad ⊓ db, erit
ab×ab=gk×gk+fd×fd, et
Erit ab ⊓ [illegible]fd.
et e contra.
hoc est
Si [illegible] a+√aa−bb
commens: a−√aa−bb.
then a commens: √aa−bb.

et si
a ⊓ √aa−bb, then
a+√aa−bb commens
to a−√aa−bb.

That is if
ba ⊓ b−a &
b ⊓ a. & if
b ⊓ a. & b+a
⊓ b−a

Si fuerint duæ rectæ lineæ inæquales AB, GK; quartæ autem parti quadrati, quod ſit à minori GK, æquale parallelogrammum ADB ad majorem AB applicetur, deficiens figurâ quadratâ, & in partes AD, DB longitudine commenſurabiles ipſam dividat, major AB tanto plus poterit quàm minor GK, quantum eſt quadratum rectæ lineæ FD ſibi longitudine commenſurabilis: Quòd ſi major AB tanto plus poſſit, quàm minor GK, quantum eſt quadratum rectæ lineæ FD ſibi longitudine commenſurabilis; quartæ autem parti quadrati, quod ſit à minori GK, æquale parallelogrammum ADB ad majorem AB applicetur, deficiens figurâ quadratâ, in partes AD, DB longitudine commenſurabiles ipſam dividet.

[a] Biſeca GK in H; & [b] fac rectang. ADB = GHq: abſcinde AF = DB. Eſtque ABq [c] = 4 ADB [d] (4 GHq, vel GKq) + FDq. Jam primò,

a 10. 1.
b 28. 6.
c 8. 2.
d conſtr. & 4. 2.

The veil of the mystery was lifted only seven years ago, when on January 13th to 15th, 1920, occurred a sale by auction at Thame Park, Oxon., of *The Greater Portion of the Contents of the Mansion, early* 19*th Century English Furniture of excellent workmanship,* . . . etc. etc. etc., *and* (in small type) *Books.* The books formed less than thirty lots, some of them being bundled up, after the manner of book-auctioneers, in lots of 40, 120, and even 200 volumes. There was nothing to indicate their importance except this, that five books are specifically mentioned " with the Autograph of Isaac Newton."

The writer of this article has seen a large number of these books, and from the evidence contained in the books themselves has no hesitation in asserting that these were actually the books of Newton's library, to which a number of others, chiefly theological, had been added by the subsequent owners. All the books had a bookplate with the Musgrave arms, and the motto " Philosophemur." This bookplate is often found pasted over another, dating evidently from the first half of the Eighteenth Century, of " Revd. Carols. Huggins. Rector. Chinnor in Com. Oxon." He was in all probability the owner of the books after Newton's death, but how they came into his possession is at present unknown.

One of the most interesting books in the collection was the presentation copy from the author to Newton of Huygens's *Horologium Oscillatorium.* There was also the *Theatrum Chemicum,* 1659-61 (6 vols.), of which Brewster relates that Newton purchased it in April, 1669, for £1 8s. 6d. It is full of notes in his handwriting, as are also many of the other alchemical books in the same collection. These notes are of importance mainly in showing that Newton was interested in the mystic element of alchemy. Several of the books were by Robert Boyle, and contained either his presentation inscription to Newton, or a note by the latter, stating that they were given to him. Nearly all were bound in a late Seventeenth or early Eighteenth Century English calf, and were, almost without exception, in remarkably good preservation. Many showed signs of use by having both top and bottom corners turned down in Newton's peculiar manner, and it must be confessed that this happened in most cases in works on theology and alchemy, subjects in which he seems to have been chiefly interested in later years.

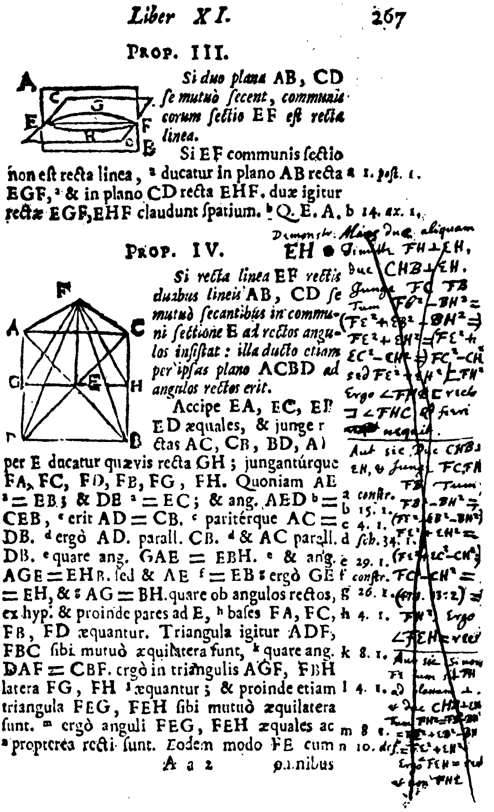
For some years after the sale, books from this collection could be seen in the catalogues of many of the London second-hand booksellers, some with inscriptions by or to Newton, but alas ! without any reference to MS. notes by him. A few found their way back into the sale rooms, and only after the sale were they found to contain Newton's notes.

The flood is now ebbing, but has not yet quite subsided, and the backwash still throws up treasures to the wary finder. Among the jetsam thus found was one of the most interesting books in the collection.

On April 20th, 1926, was sold by auction the library of a collector of chemical books, which included a number of the works dispersed at Thame. Among them were Boyle's *Essay on the Strange Subtility . . . of Effiuviums* (1673), with an inscription " For Mr. Isaac Newton, from the Author," and the *Memoirs for the Natural History of the Humane Blood* (1683-4), with an inscription by Newton, saying that it was the gift of the author. One lot consisted of the first edition of Isaac Barrow's edition of *Euclid,* in Latin (1655), which was knocked down for the modest sum of 5s. This was afterwards found to be full of notes by Newton, to which a peculiar interest attaches. Brewster relates in his *Life,* that when Newton first began to read *Euclid,* he thought the propositions self-evident, and gave up the further study as a waste of time. Isaac Barrow, his teacher, however, strongly advised him to take it up again, and this volume affords evidence that he faithfully carried out the advice. The whole book is full of corrections and notes, most of which give the propositions in algebraic notation, doubtless as a help in its study. Books V, VII, and X bear especially the mark of having received Newton's attention, while Book XI is annotated only at the beginning, and Books VI, VIII, IX, XII, and XIII, and the Data seem to have been neglected, for they hardly show any signs of use. There are also a few notes in a different and later handwriting, and its owner is evidently responsible for the thumbing of the *Pons Asinorum*

(I, 5), for it would be an insult to the memory of Newton to suggest that it was due to him.

This is probably the only relic of Newton's library at Cambridge that is still left. How many other finds there may still be hidden in the many books spread over so many quarters—most of which, it is to be feared, have now left these shores—it is impossible to say. To the collector they will afford a new interest in finding books with Newton's notes. Their discovery should not be difficult. He has to look for any books with the bookplate bearing the motto "philosophemur," and then search for the notes. The illustrations given above, containing specimens of two different styles of Newton's handwriting, will afford easy means of comparison and identification.



Liber XI. 267

PROP. III.

Si duo plana AB, CD *ſe mutuò ſecent, communis eorum ſectio* EF *eſt recta linea.*

Si EF communis ſectio non eſt recta linea, [a] ducatur in plano AB recta EGF, [a] & in plano CD recta EHF. duæ igitur rectæ EGF, EHF claudunt ſpatium. [b] Q. E. A.

a 1. *poſt.* 1.
b 14. *ax.* 1.

PROP. IV.

Si recta linea EF *rectis duabus lineis* AB, CD *ſe mutuò ſecantibus in communi ſectione* E *ad rectos angulos inſiſtat: illa ducta etiam per ipſas plano* ACBD *ad angulos rectos erit.*

Accipe EA, EC, EB, ED æquales, & junge rectas AC, CB, BD, AD; per E ducatur quævis recta GH; junganturque FA, FC, FD, FB, FG, FH. Quoniam AE [a] = EB; & DE [a] = EC; & ang. AED [b] = CEB, [c] erit AD = CB. [c] paritérque AC = DB. [d] ergò AD. parall. CB. [d] & AC parall. DB. [e] quare ang. GAE = EBH. [e] & ang. AGE = EHB. ſed & AE [f] = EB [g] ergò GE = EH, & [g] AG = BH. quare ob angulos rectos, ex hyp. & proinde pares ad E, [h] baſes FA, FC, FB, FD æquantur. Triangula igitur ADF, FBC ſibi mutuò æquilatera ſunt, [k] quare ang. DAF = CBF. ergò in triangulis AGF, FBH latera FG, FH [l] æquantur; & proinde etiam triangula FEG, FEH ſibi mutuò æquilatera ſunt. [m] ergò anguli FEG, FEH æquales ac [n] propterea recti ſunt. Eodem modo FE cum

A a 2 planibus

a *conſtr.*
b 15. 1.
c 4. 1.
d *ſch.* 34. 1.
e 29. 1.
f *conſtr.*
g 26. 1.
h 4. 1.
k 8. 1.
l 4. 1.
m 8. 1.
n 10. *def.*

Demonstr: [illegible] duc aliquam EH [illegible] FH ⊥ EH, duc CHB ⊥ EH. Junge FC FB. Tum $FB^2 - BH^2 =$ $(FE^2 + EB^2 - BH^2 =)$ $FE^2 + EH^2 =$ $(FE^2 + EC^2 - CH^2 =)$ $FC^2 - CH^2$ sed $FE^2 + EH^2$ [illegible] Ergo $\angle FHC$ recto [illegible]

Aut sic. Duc CHB ⊥ EH, & junge FC, FH, FB. Tum $FB^2 - BH^2 =$ $(FE^2 + EB^2 - BH^2)$ $FE^2 + EH^2 =$ $(FE^2 + EC^2 - CH^2)$ $FC^2 - CH^2 =$ [illegible] Ergo $\angle FEH =$ recto

Aut sic. [illegible]

PORTRAITS OF SIR ISAAC NEWTON.

BY PROF. D. E. SMITH, PH.D., LL.D.

THE number of original portraits of Newton made from life is not large, nor is the number of engravings made by artists of prominence. On the other hand, the number of reproductions of some of these works is considerable, and with the modern processes of printing it is becoming very great. It is not probable nor is it at all desirable that any complete record of the present fugitive reproductions should ever be made, and in preparing the following list none of these appear, except in the case of a few photographs from paintings or statuary.

The list records only such pieces (140) as are in the writer's collection, and could, of course, be supplemented readily by any one who should care to examine the catalogues of others who have acquired a considerable number of such portraits. It includes a fairly large amount of material listed for the sole purpose of showing the great popular appreciation of Newton's work, it being evident that such pieces have no intrinsic merit.

The engraved portraits of Newton are chiefly from paintings by Vanderbank (1694 ?-1739), Sir Godfrey Kneller (1646-1723), Enoch Seeman (1694-1744), Sir Peter Lely (1618-1680), Sir James Thornhill (1675-1734), and William Gandy, Jr. (d. 1729), and from Croker's medal.

The best known of the several Vanderbank portraits was painted in 1725 and is in possession of the Royal Society at London. It represents Newton at the age of eighty-three, dignified, attired in court dress, and uncomfortable in pose. Another Vanderbank, less often copied, and apparently painted at about the same time, is in the National Gallery in London.

Next in popularity is the well-known head by Kneller, in the collection of the Earl of Egremont. It has probably been engraved more often than any other, apparently because of the huge wig, the face having less character than appears in the portraits by Gandy, Seeman, or Vanderbank.

For real humanity, however, the painting by Gandy, executed in 1706, is one of the most noteworthy. It represents the savant in the prime of life, without a wig, and with an expression of determination tempered with kindness that is not seen in the more conventional portraits by Kneller and Vanderbank.

Next to Gandy may possibly rank Seeman, or perhaps it is safer to say the MacArdel mezzotint of his painting, the latter being a work of remarkable refinement and character.

Somewhat similar, on the score of the human element, is the Thornhill (1675-1734) painting, evidently midway in time between the work of Gandy and that of Vanderbank.

Sir Peter Lely's painting of Newton when a bachelor of arts in Trinity College is also well known, but it shows little of the character which appears in the later portraits.

If we may judge by the engravings, the Richter portrait (1714) could have had little to commend it to the historian or the artist.

A part of this list was published in *Bibliotheca Mathematica* eighteen years ago (3rd series, vol. ix. pp. 301-308), and is now revised and extended. For a list of the mezzotints the reader will naturally consult John Chaloner Smith, *British Mezzotint Portraits.*

In the descriptions the measurements are made in centimetres to the plate mark whenever one exists. If there is no plate mark they represent the extreme length and breadth of the picture to the final printed marks. The quotation marks inclose all legends upon the picture.

I. After Vanderbank.

1. Mezzotint. 32·5 × 24·4 cm. By Faber after Vanderbank. 1727. Proof before letters.

2. Mezzotint. 35·4 × 25·4 cm. " Isaacus Newton Squ^{r} Obijt 20 Mar. 172$\frac{6}{7}$ Ætatis Suæ 85, I. Vanderbank pinx. An^{o} 1726 Pro Martino Folkes Arm^{o} Cui hanc Tabulam D. D. I. Faber."

3. Engraving. 21·5 × 15 cm. " Isaacus Newton Eq. Aur. Æt. 83. I. Vanderbank pinxit 1725. Geo. Vertue Sculpsit 1726."

4. Engraving. 16·7 × 10·4 cm. " Engraved by E. Scriven. Sir Isaac Newton. From the original Picture by Vanderbank in the possession of the Royal Society."

5. Engraving. 22·5 × 15·2 cm. " Vanderbank pinx. Laderer Sculp." Scroll border.

6. Engraving. Same as 5, same legend, without scroll border.

7. Engraving. 16·2 × 9·5 cm. Sitting, $\frac{3}{4}$ length, half right. " Sir Isaac Newton, London ; Published by Hodgson & C^{o}, 10, Newgate Street."

8. Engraving. 15·9 × 11·7 cm. " Sir Isaac Newton." Same style as No. 9, different engraver.

9. Engraving. 13·1 × 9·5 cm. " Sir Isaac Newton." Same style as No. 8, different plate (type of name changed).

10. Engraving. 7·4 × 6·2 cm. " Vanderbank p^{t}. A. Smith, sc. Pub^{d} by Harrison & C^{o} April 1, 1794. Sir Isaac Newton." (Oval portrait slightly to r.) Also a biographical note.

11. Engraving. Same as No. 10, without letters.

12. Engraving. Same as No. 10, but from another plate with frame surrounding the oval.

13. Engraving. 19·8 × 11 cm. " J. Vanderbank pinxt. Pub. Aug^{t} 1, 1817 by C. G. Dyer, Compton Str^{t} Soho. Romney, sculp. Isaac Newton. Sir Isaac Newton." Full length, seated, $\frac{1}{2}$ r.

14. Same, with " G. M. Brightly, del." additional.

15. Engraving. 23·6 × 15·1 cm. " Vanderbank pinx. Ridley sc. Sir Isaac Newton Kn^{t}. President of the Royal Society 1720-1. Pub. June 25 1802 by Bunney & Gold 105 Shoe Lane."

16. Engraving. 13·1 × 9·5 cm. " R. Scott Sc. Sir Isaac Newton."

17. Engraving. 17·6 × 11·2 cm. " Literary Magazine & British Review. S^{r} Isaac Newton. Published as the Act directs 1. Dec^{r} 1788 by C. Forster N^{o} 41 Poultry."

18. Engraving. 15·4 × 6·2 cm. " Isaac Newton. Isaac Newton London : Published by Thomas Kelly, 17, Paternoster Row."

19. Engraving. 8·3 × 7·9 cm. Bust, $\frac{1}{4}$ l. " Neuton."

20. Photo-engraving of pen-and-ink drawing by Jacques Reich. Medallion, 6·2 × 4·7 cm.

21. Engraving. 7·9 × 5·6 cm. " Sir Isaac Newton."

22. Engraving. 7·7 × 5·3 cm. Bust $\frac{1}{4}$ r. " Newton."

23. Engraving. 16·2 × 12·4 cm. " T. Richomme del. E. Conguy Sc."

24. Engraving. 11·7 × 9·4 cm. "T. Richomme del. E. Conguy sc. Newton." Bust ¼ r.

25. Engraving. 24·7 × 15·1 cm. " Mme. Fournier sc. Newton. Publié par Furne, Paris." Closely resembles No. 24.

26. Engraving. 15·4 × 10·2 cm. "gest. v. F. Weber in München НЬЮТОНЪ. Newton."

27. Engraving. 24·7 × 16·6 cm. " gest. v. F. Weber in München НЬЮТОНЪ· Собственность И$_3$. 4. Пантеона." The portrait is the same as in No. 26.

28. Engraving. 11·7 × 8·9 cm. " G. Guadagnini inc: Newton."

29. Lithograph. Bust ¼ l. " Newton. Né en 1642 à Woolsthorpe (Angleterre)," and a biographical note.

30. Engraving. 17·2 × 10·7 cm. " Sir Isaac Newton K^{t}".

31. Engraving. 12 × 8·1 cm. " Sir Isaac Newton. Born at Woolsthrope, near Grantham ; in the county of Lincoln, Dec^{r} 25th 1642, expired March 20th 1727. London : William Darton ; 58 Holborn Hill, 1822."

32. Engraving. Same as 31, with " London, William Darton," etc., lacking.

33. Engraving. 22·2 × 15·1 cm. With portrait of Dr. S. Clarke.

34. Engraving. 15·6 × 9·3 cm. Bust ¼ r. " Sir Isaac Newton." Rectangular border.

35. Photograph from the painting in the National Gallery. 17·4 × 14·6 cm.

36. Coloured photo-lithograph of same. 13·3 × 8·5 cm. " Sir I. Newton. By J. Vanderbank."

37. Engraving. 14·7 × 14 cm.

II. AFTER KNELLER.

1. Mezzotint. Full wig. Front. 34·2 × 25·5 cm. " Isaacus Newton Eq. Aur. G. Kneller. Eques. pinx. I. Smith Fecit et ex. 1712."

2. Engraving. 48·8 × 39·4 cm. " On the portrait plate Jsaacvs Newtonvs. G. Kneller f. 1689." Beneath : " London : published Jan^{y} 1st 1867, by Moore, M^{c}Queen & Company, Limited, 26 Berners Street, Oxford Street, & 22 Rue de Dunkerque, Paris [.] To the right Hon^{ble} the Earl of Portsmouth the Collateral descendant of Sir Isaac Newton, This portrait of that eminent Philosopher, Engraved by Tho^{s} Oldham Barlow, from the original picture by Kneller in his Lordships possession, handed down from Newton himself and representing that great man at the most interesting period of his life ; Is inscribed by his Lordship's servants, The Publishers."

3. Engraving. 14·3 × 9·8 cm. " Sir Isaac Newton. Engraved by M^{r} S. Freeman from the Original Painting by Sir Godfrey Kneller. Published by Arch^{d} Fullarton & Co. Glasgow."

4. Engraving. 15·8 × 10·1 cm. " W. Holl. Isaac Newton A. Fullarton & C^{o} London & Edinburgh." Half length, with compasses.

5. Engraving. 24·4 × 16·2 cm. " S^{r} Isaac Newton. G. Kneller pinx^{t} Goldar Sculp. In the possession of John Conduit Esq^{r}."

6. Engraving. 20·6 × 15·2 cm. " Engraved by W. T. Fry. Sir Isaac Newton. Ob. 1727. From the original of Kneller, in the collection of The Right Hon^{ble} the Earl of Egremont. John Tallis & Company, London and New York."

7. Engraving. 17·8 × 10·0 cm. " Engraved by W. T. Fry. Sir Isaac Newton. Ob. 1727. From the original of Kneller, in the collection of The Right Hon^{ble} the Earl of Egremont. London, Published Aug^{t} 1, 1831 ; by Harding & Lepard, Pall Mall East."

8. Engraving. 14·3 × 7·8 cm. "Desiné d'après le Portrait original de Sir G. Kneller, et Gravé par Ambroise Tardieu. Sir Isaac Newton (Physicien et Philosophe), Président de la société royale de Londres. Né à Woolsthorpe dans le Comté de Lincoln le 25 Décembre 1642. Mort à Londres le 20 Mars 1727."

9. Engraving. 17·8 × 11·1 cm. "G. Kneller pinx^{t}. W^{m} Sharp sculp^{t}. S^{r} Isaac Newton. Published by G. Kearsly, N^{o} 46 Fleet Street."

10. Engraving. 36·9 × 23·2 cm. "S^{r} Isaac Newton. I. Houbraken sculpsit. In the possession of John Conduit Esq^{r}. G. Kneller pinx."

11. Engraving. 12·7 × 5·2 cm. "Painted by Sir G. Kneller. Sir Isaac Newton. Engraved by Edward Smith. London Pub^{d} for the Proprietor, March, 1821."

12. Engraving. 24·3 × 16·1 cm. "Sir Godfrey Kneller. Robert C. Bell. Isaac Newton." The frontispiece of Brewster's *Memoirs*, vol. i.

13. Engraving. 11·2 × 6·9 cm. "Engraved by J. Mc.Gahey. Sir Isaac Newton. Published by J. Mason. 14, City Road & 66, Paternoster Row."

14. Engraving. 17·3 × 11·9 cm. "Isaac Newton. Engraved by R. Page for the Encyclopædia Londinensis 1818." Name in autograph facsimile.

15. Engraving. 19·7 × 10·0 cm. "Engraved for the Universal Magazine. S^{r} Isaac Newton. From a painting after S^{r} Godfrey Kneller. Publish'd according to Act of Parliament, for I Hinton at the King's Arms, in S^{t} Pauls Church Yard. 1748."

16. Engraving. 22·1 × 14·8 cm. "Isaacus Newton Eq: Aur. Kraus. Sc."

17. Engraving. 11·8 × 7·5 cm. "Kneller Pinx. Phillibrown Sc. Sir Isaac Newton. Ob. 1727."

18. Engraving. 13·3 × 8·6 cm. "Sir Isaac Newton."

19. Engraving. 10·8 × 6·4 cm. "Dieu dit que Newton soit et la lumière fût. Pope Trad. p^{r} Voltaire."

20. Engraving. 17·5 × 9·4 cm. "Eng^{d} by J. Scott Glasgow. Sir Isaac Newton. Published by William Collins Glasgow."

21. Engraving. 8·1 × 6·6 cm. Half length ¼ r. "Sir G. Kneller. W. C. Edwards. Sir Isaac Newton[.] Published by John Murray Albemarle Street. 1831."

22. Engraving. 12·7 × 8·1 cm. Full wig. Bust, front. "Isaac Newton. né en 1643 mort en 1726."

23. Engraving. 20·7 × 15 cm. "Isaac Newton. né en 1643 mort en 1726 J. Blanchoir (?) in. et sculp." Same as No. 22 but inclosed in engraved border (Blanchoir sculp.). Printed in reddish brown.

24. Engraving. 9·4 × 7·6 cm. "S^{r} Isaac Newton. S^{r} Isaac Newton." In oval.

25. Engraving. Bust ¼ to l. 9·1 × 6·9 cm.

26. Engraving. Same as No. 25, but with legend: "Newton."

27. Engraving. 18·1 × 11·6 cm. In an oval. Front. "des Rais del. Ex Biblioth^{ca}. Regia. Le Cœur Sculp^{t}. Newton."

28. Engraving. 15·3 × 10 cm. Bust, front, in oval.

III. AFTER SEEMAN.

1. Mezzotint. 22·5 × 17·5 cm. Natural hair. Bust. Front. Proof before letters. Signed, "J. MacArdel." One of the finest portraits we have.

2. Mezzotint. "Sir Isaac Newton. Drawn and scraped MDCCLX by James Macardel from an original portrait painted by Enoch Seeman now in the

possession of Thomas Hollis F.R. and A.SS. Les Italiens ces peuples ingenieux-encore. Ode Sur La mort de Madâme de Bareith avec une lettre par Mons. de Voltaire." Same plate as No. 1.

3. Engraving. 20·5 × 13·4 cm. "Nach gleichx. Bild v. E. Seemann. Baumann sc. Isaak Newton. Verlag der Kunst-Anstalt des Oestr. Lloyd in Triest."

4. Engraving. 22·5 × 16 cm. "J. Swaine. Sculp. Sir Isaac Newton. From an original by Enoch Seeman. *another of the managers Js Newton. Js. Newton.* [Italics in autograph facsimile.] His autographs from the Originals in the Possession of John Thane."

5. Engraving. 23·2 × 15·5 cm. "E. Seeman, p^{t}. Monsaldi Sc."

6. Engraving. 25 × 16·3 cm. Bust in border surmounted by Pallas Athena and two Graces. "Isaac Newton." Below, facsimile signature. "Engraved by John Halpin, from a painting by Enoch Seeman. London: Published by Thomas Kelly, 17, Paternoster Row, 1830."

7. Engraving. Two portraits in ovals on the same plate. 20 × 15 cm. Busts, slightly to r.

8. Lithograph. 16·6 × 10 cm. "C. Farcy. Lith. de Langlumé." Underneath, 3 books: "Attract. Calcul Differ." Below: "Newton." Bust, front.

9. Engraving in oval. 15·6 × 11·3 cm. From a pen-and-ink drawing. "Neuton. N. 1."

10. Engraving. 14·1 × 8·5 cm. In oval. "Isaac Newton, Né à Woolstrop, en 1642. Mort le 20 Mars 1727."

11. Engraving. Same as the preceding except for the legend. Plate mark 20·3 × 13·1 cm. "Newton." Below, "C. huton." At bottom: "à Paris, chez Ménard & Desenne, Rue Git-le-Cœur N^{o} 8."

IV. After Richter.

1. Engraving. Medallion. 12·5 × 8·8 cm. "Richter pinx. 1714. Sir Isaac Newton. from a Miniature picture of the same size. London, Publish'd by C. Taylor . . . , Nov^{r} 1, 1785."

2. Engraving. The same as No. 1, but without the legend "from a miniature . . . 1785." Printed in reddish brown.

V. After Roëttier's Drawings.

1. Engraving. 16·2 × 10 cm. "Tome XXXI. Page 127. Hist. D'Angleterre. Newton. Roëttiers del^{t} London direx^{t}."

2. Engraving. 12·4 × 6·1 cm. "Newton. Drawn by Roëttiers. Engraved by G. Cooke. London. Publish'd by Vernor, Hood & Sharpe, Poultry, 1807."

3. Engraving. 15·1 × 10·9 cm. "Roëttiers del Ravenet Sculp. Isaac Neuton. Mort à Londres, le 20. Mars 1727. Agé de 85 Ans. A Paris chés Odieuvre M^{d} déstampes, Quai de l'Ecole vis-à-vis la Samaritaine à la belle Image. C.P.R." Head in oval, profile, to l.

4. Engraving. Same as No. 3, without the line "A Paris . . . C.P.R."

5. Engraving. 20·3 × 15 cm. Head in oval, profile, to l. "Newton. Dessiné et Gravé par Aug. S Aubin."

6. Engraving. 14·1 × 10·6 cm. Profile to l. Above,

"Nature and nature's laws were hid in night:
God said: let Newton be! and all was light."

Below, "Sir Isaac Newton
Né le 25 décembre 1642, décédé le 20 Mars 1727
Les ténèbres régnaient sur la nature entière
Dieu dit: que Newton soit! et tout devint lumière.

7. Engraving. 10·7 × 6·2 cm. Profile to r. "Per N. Bettoni, F. Zuliani in."

8. Engraving. 10·9 × 6·6 cm. "Newton. Zeelander sc."

VI. After Thornhill.

1. Engraving. 34·7 × 24·6 cm. "Isaacus Newton Eq. Aura. I. Thornhill Eq. pinx. Simon sec."

2. Photograph. 12·4 × 10·1 cm. "Sir Isaac Newton. By Sir James Thornhill. The Property of Trinity College, Cambridge. No. 35. Published by The Arundel Society, 24, Old Bond Street."

3. Engraving. 18·1 × 11·4 cm. Bust in oval ¼ l. "Sr Jsaac Newton, Kt".

VII. After Gandy.

1. Lithograph. 36·6 × 27 cm. "Ætatis Suæ 64. 1706. Drawn on Stone by G. B. Black, 1848. Painted by William Gandy, Junr 1706. Day & Son, Lithrs to the Queen. Sir Isaac Newton, From an original painting 30 in. by 26 in. formerly in the possession of John Abel Walter, Esq. of Ashley Mount, near Lymington. Proof."

VIII. After Lely.

1. Engraving. 35·6 × 24·5 cm. "Sir Isaac Newton, when Bachelor of Arts in Trinity College, Cambridge. Engraved by Bt Reading from a head painted by Sir Peter Lely in the Possession of the Right Honorable Lord Viscount Cremorne. Pubd as the Act directs Jany 1799. by Bt Reading and Sold by Wm Richardson York House Strand."

2. Engraving. 17·4 × 10·3 cm. "Sir Isaac Newton, when Bachelor of Arts in Trinity College, Cambridge. Engraved by J. T. Wedgwood, from the Original Picture Painted by Sir Peter Lely, in the possession of The Right Honorable Lord Viscount Cremorne."

3. Engraving. Same as No. 2 with addition preceding the legend: "London, Published Decr 1st, 1808 by the Rev. T. Baseley."

4. Engraving. Bust. Half left. 10·8 × 7·2 cm. "Engd by J. Mc.Gahey. Isaac Newton, B.A. Published by J. Mason, 14, City Road, & 66, Paternoster Row."

5. Photograph. Half length. Half right. 12·3 × 10·1 cm. "Sir Isaac Newton, Kt. By Sir Peter Lely. The Property of Earl of Dartrey. No. 23. Published by The Arundel Society, 24, Old Bond St."

6. Engraving. Bust. Half right. 17·5 × 11·4 cm. In oval. "Engraved for Mackenzie & Dent's Select Biography. T. Clerk Sculpt. Sir Isaac Newton."

IX. After Croker's Medals.

1. Engraving. 13·9 × 9·8 cm. "Ic. P. Dupin Sculpsit. Isaac Neuton Gravé d'Après la Medaille Ic." (Other words not legible.)

2. Engraving. Same as No. 1, but without the illegible words.

3. Engraving. Ob. and rev. of the Croker medal, each 5·9 cm. in diameter. Illustration from a book.

4. Engraving. Circular medallion, 9·9 cm. in diameter. "Sir. Isaac. Newton. Born. MDCXLII. Died. MDCCXXVI." (1726 O.S.)

5. Engraving. In a circle, 8·4 cm. in diameter. "Newton." Probably suggested by Croker's medals.

X. Miscellaneous Painters and Engravers.

1. Photograph. 13·2 × 10·7 cm. "Sir Isaac Newton. By Lewis Crosse. The Property of the Marquis of Exeter. No. 29. Published by the Arundel Society, 24, Old Bond Street."

2. Photograph. 15·7 × 13·8 cm. From a painting in the National Portrait Gallery, London. "N.P.G. 1395. Sir Isaac Newton. Robert Walker ?"

3. Engraving. 30·2 × 23·7 cm. "Js. Newton 'hypotheses non fingo.' Engraved by J. Outrim from the original Drawing in the Pepysian Collection at Cambridge."

4. Engraving. 14·6 × 10·3 cm. "Isaac Neuton fameux Mathematicien né en Angleterre et il y est mort. Desrochers ex."

> "De ce physicien, le Sublime genie,
> De la nature entière, apenétré les loix.
> A ses profonds calculs, elle senble asservie,
> Et ne S'expliquer plus que par sa seule voix. R."

5. Engraving. Same as 4 without "Desrochers ex."

6. Engraving. 12·2 × 7·4 cm. "Aveline sculp. Sir Isaac Newton."

7. Engraving. 16 × 9·6 cm. "Isaac Newton. Sysang Sc."

XI. After Busts and Statues.

1. Engraving. 26·6 × 16·1 cm. "I. Allen delin^{t}. E. Scriven Sculp^{t}. Sir Isaac Newton. From a Bust at the Royal Observatory, Greenwich."

2. Engraving. 15·5 × 8·9 cm. "F. Mackenzie. J. Le Keux. Statue of Sir Isaac Newton, in the Ante Chapel, Trinity College. Published March 1st 1838; by C. Tilt, Fleet Street: & J. Le Keux, Harmondsworth."

3. Photograph. 20·3 × 15 cm. Statue by Roubilliac (Roubiliac) in Trinity College. "Newton. Qui genus humanum ingenio superavit." The engraving appears as the frontispiece of Brewster's *Memoirs*, vol. ii. See also No. 4 below.

4. Engraving. 12·5 × 7·2 cm. Statue in Trinity College. "Roubiliac. Normand fils. Newton."

5. Engraving. 16·4 × 9·3 cm. Newton's Monument in Westminster Abbey. "S^{r} Isaac Newton's Monument."

6. Photograph. 17·7 × 10·7 cm. "Newton's Monument, W.A."

7. Photograph of marble bust. 16·3 × 12·5 cm.

8. Engraving. In an oval, 4·7 × 3·8. "Newton, from a bust." Poor woodcut.

9. Mezzotint. 30·9 × 23·1 cm. From a bust, as No. 7.

10. Engraving. 10·9 × 8·8 cm. Head from the same statue as No. 7. "Lips Sc."

Miscellaneous. Artists Unknown.

1. Engraving. 13·1 × 8·6 cm. (With portraits of Leibniz and Samuel Clark.) "Isaacus Newton. G. Wil. von Leibnitz. Sam. Clark."

2. Engraving. 21·3 × 11·7 cm. (With portraits of Locke, Boyle, and Bolingbroke.) "Smollet Eminent Men. Joh. Locke Sir Isaac Newton Hon^{b} Rob^{t} Boyle. Viscount Bolingbro. Edwards sc. Vol. II. Published by R. Scholey, 46 Paternoster Row, April 21st 1806."

3. Lithograph. 15·6 × 10·6 cm. "Newton. Publié par Soetens & Fils, à La Haye. G.F.E." Very poor portrait. Two-line border.

4. Engraving. 17·5 × 11·9 cm. Eight medallions, Queen Anne in the centre surrounded by leading men of her time. Bust of Newton at the top. Before names were inserted.

5. Engraving. 17·5 × 10·6 cm. In oval, ½ r. "Is. Newton 'hypotheses non fingo.'" From a portrait in the Pepysian Collection at Cambridge.

6. Engraving. 11·9 × 8·1 cm. "Sir Isaac Newton. Born Dec. 25, 1642, O.S., at Woolsthorpe, England; died March 20, 1726-27, O.S., at London." Same source as No. 5.

7. Engraving. 10·3 × 7·4 cm. From the same source as No. 5.

8. Engraving. 14·4 × 8·8 cm. "George Cruickshank, Sir Isaac Newton's Courtship." Fanciful. The Newton portrait is suggested by Kneller's work.

9. Engraving. 26·9 × 20 cm. "Romney Pinx, Meadow Sculp. Newton with the prism." Purely fanciful. No resemblance to Newton.

10. Engraving on wood. 16·6 × 11·3 cm. "Sir Isaac Newton as a Boy." Half length, reclining figure, fanciful. New York, Harpers, 1881.

11. Engraving. 22·6 × 15 cm.

12-31. 19 photographic reproductions of medals of Newton described in the list of medals. Printed, Boston, 1912.

32. Photograph of his death mask, circular. 4·4 cm. in diameter. See Brewster's *Memoirs*, vol. ii. p. 415. The original cast was by Roubilliac.

[It may be as well to state that the legends are actually copied. W.J.G.]

THE PORTRAIT MEDALS OF NEWTON.

BY PROF. D. E. SMITH, Ph.D., LL.D.

IT is quite probable that more portrait medals, coins, and tokens have been struck in honour of Newton than of any other mathematician who ever lived. This is due in part to Newton's connection with the mint, but even aside from the coins or tokens struck in 1793, the number seems to exceed that of any other except, perhaps, Galileo.

The list given below is probably quite incomplete. It simply describes those that, with three exceptions, are in the writer's collection.

1. Bronze. Ob., bust by John Croker, to l. "Isaacvs Newtonvs." "I.C." Rev., a seated figure, winged head, holding a tablet with a plan of the solar system. "Felix. Cognoscere. Cavsas / M.DCC.XXVI./" The date of death (1726) is according to the old style, still in use at that time in England. 51 mm. Struck at the Tower in 1731. It is one of the best of the Newton medals.

2. Same as No. 1, but in silver.

3. Bronze. Ob., bust by Jacques Roëttiers, to r. "Isaacvs Newtonvs." "Iac. Roëttiers." Rev., a seated figure somewhat similar to that in No. 1, surrounded by a quotation from Seneca (*Quaest. Nat.* Lib. vii, cap. 26),—that someone would arise who should demonstrate in what parts of the heavens the comets move. In execution it is perhaps the best of the Newton medals, but in portraiture it is probably not so good as Croker's work. 50 mm.

4. Bronze. Ob., bust to r. by Roëttiers. "Isaacvs Newtonivs" "Jac. Roëttiers." Rev., "Quæritur / Huic / Alius / Verg. Æneid : / M.DCC.LXXIV." 54 mm. The significance of the quotation is seen from the context in the *Æneid* : "Such was Dares, who raised his lofty head in the first contest, and showed his broad shoulders, and extending his arms he tosses them about alternately, and beats the air with blows. Another is sought for him, nor did any one from so great a band dare approach the man, and draw the gauntlets on his hands."

5. Same as No. 4, but in silver.

6. Bronze. Ob., bust to l. by Petit. "Isaacus Newtonius." "Petit. F." Rev., "Natus / Volstropii / in Anglia / An. M.DC.XLII. / Obiit / An. M.DCCXXVII." 41 mm. One of a series of medallic portraits executed in Paris about a century ago, none of which bore much resemblance to the person named.

7. Bronze. Ob., bust ¾ to r. "Isaacus Newtonius." Rev., wreath surrounding inscription : "Eq. Aur. / Philosophus. / Obiit 31. Mart. / 1727. / Natus Annos / 85." 33 mm. An inferior piece of work. Possibly the artist was trying to follow one of Vanderbank's portraits, but the face is entirely wanting in strength or in resemblance to Newton.

8. Same as No. 7, but in silver.

9. Bronze. Ob., bust by Dassier, to r. "Isaacus Newtonius. /" "I. Dassier F." Rev., his tomb in Westminster Abbey. "Nat. 1642. / M. 1726. /" 42 mm.

10. Same as No. 9, but in silver.

11. Same as No. 9, but in bronze gilt.

12. Same as No. 9, but a poor cast from the original, in bronze.

13. Silver. Ob., bust to r. by George Mills. "The Astronomical Society of London Instituted MDCCCXX. Nubem pellente mathesi." "Mills F." Rev., Herschel's telescope, "Quicquid nitet notandum." "Struck by permission of the Council for the Collection of W. D. Haggard Esq., M.A.S. 1829." 38 mm. These medals were bestowed by the Society from time to time upon distinguished astronomers. This particular piece was, as stated, struck for a private collection. The portrait bust is well executed, but it does not bear much resemblance to the contemporary paintings of Newton.

14. Silver. Ob., bust to r. by W. Wyon. "Royal Astronomical Society Inst. MDCCCXX. Nubem pellente mathesi." "Newton." "W. Wyon, ARA. Mint." Ob., similar to that of No. 13, but better executed. This piece was struck specially in 1837 for Mr. Haggard's collection. When the Astronomical Society of London received its Royal Charter in 1831, No. 13 was changed to this medal. The bust is the noblest and most faithful to the original of any of the others except, perhaps, the Croker and Roëttiers ones (Nos. 1-4).

15. Tin. Ob., bust to r. "Sir Isaac Newton." "L.B." "Died 20 March 1727." Rev., "Sir Isaac was born at Woolsthorpe in the Soke of Grantham Decr 25th 1642 and educated at the Free Grammar School of King Edward the Sixth. Grantham." 45 mm. The work is mediocre.

16. Tin. Ob., bust to r. A calendar medal. "A Calendar 1822. S. Isaac Newton." Rev., calendar. 42 mm. The bust is poorly executed.

17-22. Bronze half-pennies, private pieces, allowed to be issued in 1793, bearing Newton's portrait. Ob., bust to l. "Sr. Isaac Newton." Rev., "Half Penny / 1793." Design in the centre. 27 mm. All of the same design, but not all struck from the same die, as slight differences in five show, and the sixth is from different grade of metal. Four have milled edges. Two have inscriptions on the edges, one reading: "Payable in Hull and in London," and the other: "Payable in London Bristol & Lancaster."

23. Bronze. A farthing struck at the mint in 1793. Ob., bust to l., of the same design as in Nos. 17-22. "Sr. Isaac Newton." Rev., "Farthing / 1793." Design in the centre. 21 mm.

24. Bronze. Same as No. 23, but 0·5 mm. larger. They seem to have been struck from the same die, but this is not certain. 21·5 mm.

25. Bronze. A farthing struck in 1793. Ob., bust to l., of the same design as in Nos. 23 and 24, but not from either of the above dies. Legend, "Is. Newton." Rev., "Farthing." Monogram F.H. (T.H. ?). "1793."

26. Bronze. A farthing struck in 1793. Ob., bust to l., differing considerably from those in Nos. 23-25. "Isaac Newton." Rev., "Farthing." Britannia seated. "1793." 20·5 mm.

27. Bronze. A farthing struck in 1793. Ob., bust to l. "Ic. Newton." Rev., "Farthing." Britannia seated. "1793." 20·5 mm.

28. Bronze. Similar to No. 27, but not from the same die. 21 mm.

29. Bronze. Similar to Nos. 27 and 28, but not from the same die. 22 mm.

EPITAPH.

M. S.

Incomparabilis viri Domini Isaaci Newtoni, Equitis Aurati,
sui saeculi Philosophorum facilè principis;
qui summam propter probitatem morum et egregia merita
per plures annos Regiae fuit rei monetariae Britan. praefectus;
ob sophiam Soc. Regiae Londini Praesidens;
ob amorem in natale solum Lindi Colinense
Soc. Generosae Spaldingiis socius;
Philosophiam Naturalem,
fabellis verborumque portentis deformatam,
veris clarisque idaeis instruxit;
per orbes inextricabiles vorticesque insanos errantem
in finibus certis conclusit;
vacillantem et pedem figere nesciam
in firmissimo experimentorum fundamento constituit,
et in aeternum stabilivit;
eam denique Theologiae ancillantem et de Atheismo triumphantem
orbi exhibuit.
Humanae scientiae limites novit
quousque progredi datum sit,
et, quod magis, ubi sistendum.
Hinc uti se scire non superbiit,
ita nescire non erubruit.
Nullius opinioni mancipatus,
minimè omnium suae;
Veri indagator et arbiter;
Falsi nihil aut intellectui ejus fraudem
aut voluntati vim facere potuit;
adeo illum mens solers animusque integer
undique tutum praestitere.
Post longam annorum seriem
in doctrinae studiis promovendis
erroribusque detegendis
feliciter exactam,
placidè tandem emigravit
ad veri rectique originem
fontemque perennem
A. S. H. 1727.

Supposed by rev. and learned Dr. Francis Lockier, Dean of Peterborough; reduced into an epitaph by the Sec. of the Gentleman's Society at Spalding; given to the Hon. Sir Rich. Ellys, Bart., an acquaintance of Mr. Conduitt. (Nichols, *Lit. Anecdotes*, *18th Century*, v. p. 104.)

PRINTED IN GREAT BRITAIN
BY ROBERT MACLEHOSE AND CO. LTD.
THE UNIVERSITY PRESS, GLASGOW